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PART I.

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MDCCCXXIV.

ADVERTISEMENT.

The Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions;* which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to

be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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The Meteorological Journal will appear in the Second Part of the Philosophical Transactions.

THE President and Council of the Royal Society adjudged the Medal on Sir Godfrey Copley's Donation, to John Pond, Esq. Astronomer Royal, for his various Communications to the Royal Society.

PHILOSOPHICAL

TRANSACTIONS.

I. *The Croonian Lecture. On the internal structure of the Human Brain, when examined in the microscope, as compared with that of Fishes, Insects and Worms. By Sir* Everard Home, *Bart. V.P.R.S.*

Read November 20, 1823.

At the time this Lecture was instituted for the discovery of the principle on which muscular motion depends, the principle was supposed to be inherent in the muscular fibre itself: the numerous dissertations, therefore, which are registered in the Philosophical Transactions, are in general so many investigations into the properties of muscular fibres.

This part of the subject may be considered as completely exhausted, although the principle on which the motion depends is not yet made out. This leads me to believe that we must extend our enquiries to the structure of the brain and nerves before we can arrive at it.

It is now, I trust, universally allowed by physiologists, that muscular motion cannot take place in living animals without the aid of the medullary structure of which the brain and nerves are composed.

Every enquiry into the more minute parts of the brain and nerves, as well as their ganglions, must be carried on upon the field of the microscope, and Mr. BAUER, in consequence of the many valuable discoveries which have been made by his skilful use of that instrument, has too much zeal for science to withdraw himself from such pursuits.

Considerable advance has been already made in this enquiry by Mr. BAUER's observations on the component parts of the blood, the formation of the embryo of the chick, and the component parts of the brain.

These observations are registered in the Philosophical Transactions, and the communications have been of so recent a date, as not to require that I should do more in this place than refer to them.

In the present Lecture, it is intended to pursue still farther the anatomy of the human brain, and to compare it with that of fishes, insects and worms; in the hope that by the establishment of such a series of facts, we may throw light on the connection between the action of the nerves and the motion of the muscles.

The following circumstances explain the difficulties that are met with in the examination of the structure of the human brain.

The transparent elastic matter readily dissolves in water, and when in solution the globules become one confused mass.

The cortical substance containing serum, when it coagulates, is different in density from the medullary structure, and readily separates from it; the serum also in drying cracks into regular figures, giving it an appearance of a net work which is artificial.

The globules and elastic substance being in different proportions in different parts of the brain, have an influence upon the appearance put on by the coagulum, which is a source of deception, and makes the coagulum tear more readily in some directions than others, as if fibres in those directions existed, which is not the case.

The parts not being of one uniform density, makes it impossible for the sharpest knife to cut evenly; some of the globules must therefore be displaced, while others are not.

When the brain is immersed in alcohol and rendered solid, all these deviations from the natural appearance must be encreased in a greater degree, since the elastic substance is rendered opaque by coagulation, and all appearance of globules lost.

To obviate these sources of error, I requested Mr. BAUER to examine a small portion of the human brain in a recent state, composed of cortical and medullary structure, which had been immersed in distilled water: the surface of the elastic substance it is true, was dissolved, but what remained completely preserved its transparency; the appearance the surface put on when magnified twenty-five times, is seen in the annexed drawing, rows of globules from the circumference of the cortical substance are passing unbroken in straight lines into the substance of the medullary portion, which could not have been detected in any other mode of investigation.

I shall not prosecute farther this part of my subject on the present occasion, but proceed to the consideration of the brain of fishes, (that of quadrupeds and birds being in its structure so similar to the human brain, as not to require any notice of its peculiarities to be taken).

In the representation of the brain of the Tench, which is annexed, there is evidently a smaller quantity both of medullary and cortical substance in proportion to the size of the animal than in the bird, and its form is less compact, being made up of spherical nodules, medullary on the surface, and internally cortical: the basis of the brain is also nodulated, and in the centre is an oval cavity. The nodules are upon so small a scale that their internal cavities are not to be distinguished, but in the squalus maximus they are very conspicuous. I am now to point out the peculiarities of the brain in insects and worms; but cannot tread upon the same ground on which Swammerdam has preceded me, without paying a tribute of praise to that great man, who, labouring under such disadvantages as he must have done, in the age in which he lived, has performed so much, and in many instances has left nothing for those who follow him, but to bear testimony to the correctness of his representations and judgement.

There are some points in which he gave way to public opinion, and did not disbelieve what every one said must be true. I allude to his attempt to represent the eye of the garden snail at the point of the horn, which does not exist. He found black retemucosum, which he mistook for nigrum pigmentum, and a pellucid part which he took for cornea. To show this fallacy, I have annexed Mr. Bauer's representation of these parts. Swammerdam has given a faithful representation of the nerve, which might have undeceived him, it having no resemblance to other optic nerves, but being like those commonly met with going to tentacula.

It is curious, that long as has been the intervening period of time between Swammerdam and Bauer, no one has

arrived at a like excellence in the use of the microscope; for certainly POLI (however splendid his plates) cannot be put in competition with either of them.

When SWAMMERDAM died, and no one found himself equal to succeed him, a report was raised that his microscope was of a peculiar kind, and the mode of using it was lost at his death; so it is now with BAUER. Many applications are made to the mathematical instrument makers for a BAUER's microscope, by those who are not willing to believe it is their inability, and not the fault in the microscope, that prevents their arriving at his excellence.

In all the insect tribe I have examined, the brain is formed upon the same general principle, but very different from that of fishes; the brain is in one mass; it is too small to admit of a particular description, but contains globules; and from the readiness with which it dissolves upon exposure, there is no doubt of there being a fluid contained in it. Besides this, which is admitted to be the brain of the insect, there is another substance connected to it by means of two chords. This second part has been, I believe, usually called the first ganglion, but, when accurately examined, it is similar in its texture to the brain: the two chords which unite them are not properly nerves, since they are upon their first exposure turgid, but soon collapse. These two substances with their uniting chords form a circle, and surround the œsophagus; from the upper mass go off the optic nerves, those to the tentacula, tongue, &c.

From the lower mass go off the nerves to the upper extremities.

I shall therefore consider the upper as the brain, the lower as the medulla spinalis.

Below this is a regular line of ganglions, properly so called, being made up of a congeries of nerves, as the ganglions in the human body are now admitted to be.

The brain appears to be made up of two lobes. The mass I call medulla spinalis, is also made up of two portions, united together by the two lateral chords.

The ganglions down the body of the animal are united together by a double nerve.

The annexed drawings show this structure better than can be explained by verbal description. Among the insect tribe the brain of the Humble Bee stands first, as being largest in proportion to the size of the body of the insect. Swammerdam has given a representation of the Bee; it is in general correct, but not so in respect to the optic nerves.

The Moth and Caterpillar have the same kind of brain, medulla spinalis, and series of ganglions, as in the Bee: the parts in the Caterpillar are nearly of the same size as in the Moth, but in both they are smaller than in the Bee. Swammerdam has given a correct representation of these parts in the Caterpillar of the Silk-worm, but none of the Moth.

The Lobster is similar in the structure of all these parts to the Bee, and although they are smaller in proportion to the size of the animal, they are still so large as to be readily seen, and explain what is not so distinct in the smaller insects.

The Earth-worm has a brain and nerves formed upon a smaller scale, but made up of the same parts.

In the Garden Snail, the brain and medulla spinalis are upon the whole larger in proportion to the size of the animal than in the Bee; but in this animal, there are no ganglions, which may account for those parts being so large.

The clear and distinct representations which Mr. Bauer has given of these very minute objects, has made it unnecessary for me to take up the time of the Society in giving detailed descriptions of the different parts; indeed any thing that is to be said, had better be stated in the explanation of the drawings, than connected with the general remarks which form the Lecture itself.

The Snail having a brain of the same kind as the Bee, and the medulla spinalis having a similar structure, while the series of ganglions is wholly wanting, forms one of the most curious parts of this investigation. Having ascertained that, in all the animals, the structure of whose nervous system has been explained in the present Lecture, the brain is a distinct organ, varying in size it is true, till at last it is scarcely distinctly visible to the naked eye, but when examined in the microscope, found to consist of globules and elastic transparent matter, and more or less of a fluid, similar to the brain of animals of the higher orders; that there is also at some distance from the brain, a second substance of similar structure, connected with the brain by two lateral chords; and that this second part gives off the nerves that go to the different muscular structures of the body; I consider myself borne out in the opinion that this part answers the same purpose as the medulla spinalis.

The ganglions which form a chain connected so beautifully together by a double nerve, must be considered to have the same uses, whatever they are, as the ganglions in the human body, being equally composed of a congeries of nerves. These are facts, which if they are allowed to be clearly made out, form an addition to our knowledge, and give confirmation to opinions not before satisfactorily established.

Here I shall conclude the present Lecture, neither Mr. BAUER's time nor my own having admitted of our proceeding farther in this curious and interesting anatomical investigation, which I am not without hope, in the course of another year, of our rendering as complete as microscopical observations admit of, by examining the nervous system of a class of animals in which the existence of a brain has not been ascertained.

Till that is done, I must postpone any physiological remarks connected with the subject, all the facts belonging to it not having been determined.

EXPLANATION OF THE PLATES.

PLATE I.

Fig. 1. A small portion of the human cerebrum in a recent state, which had been immersed in distilled water; magnified five diameters.

Fig. 2. A smaller portion, magnified twenty-five diameters; showing the arrangement of the globules, in straight lines, which pass uninterruptedly across the cortical substance into the medullary.

Fig. 3. A still smaller portion, magnified two hundred diameters, by which means the globules are rendered conspicuous.

Fig. 4. The brain of a tench, of the natural size. The cranium is removed, and the upper surface of the brain exposed to view.

Fig. 5. The under surface of the same brain.

Fig. 6. A horizontal section of the same brain; showing that the nodules have a cortical substance internally, and a medullary on the outer part.

Fig. 7. The cavity in the tench's brain.

Fig. 8. The brain and nerves of the humble bee; magnified ten diameters.

The brain is of a truncated oval form; it gives off the nerves to the eyes and feelers; its internal structure made up of globules. The substance, corresponding in its uses to that of the medulla spinalis, is nodulated on its external surface, connected to the brain by two long chords, which differ from nerves in collapsing soon after being exposed; these I shall call crura cerebri: the lower portion of this nodulated structure corresponds to the medulla spinalis, and, in its internal structure, resembles the brain.

The two nerves that go down, connected at certain distances by small nodules, are different from nerves in being more pulpy, and the nodules themselves are composed of congeries of nerves, similar to the ganglions in the human body, of which there are many excellent representations before the public, which makes it unnecessary to show their structure upon this occasion.

Fig. 9. The brain and nerves of the garden snail; magnified four diameters.

In this animal the brain is made up of two apparently equal portions. As the appearance at the termination of the two large horns resembles eyes, and Swammerdam has attempted to delineate the different parts of the organs: Mr. Bauer has shown the two nerves in different states.

The medulla spinalis forms a larger mass than the brain, but equally made up of two distinct parts. From the upper edge of this mass, there is an azagos branch going directly

upward to the muscles of the tongue, beyond which are the glands of the mouth, and the œsophagus cut through.

This nerve, so similar to the recurrent in the human body, only differing in being single, justifies me in having given the name of spinal marrow to the part that gives it off.

Fig. 10. The point of one of the large horns, magnified fifty diameters; to show that the external point of its termination in no respect resembles a cornea, but consists of five bundles of nervous filaments, the terminations of the branches of the nerve.

Plate II.

Fig. 1. The brain, spinal marrow, ganglions, and nerves of the moth of the silk-worm; magnified ten diameters.

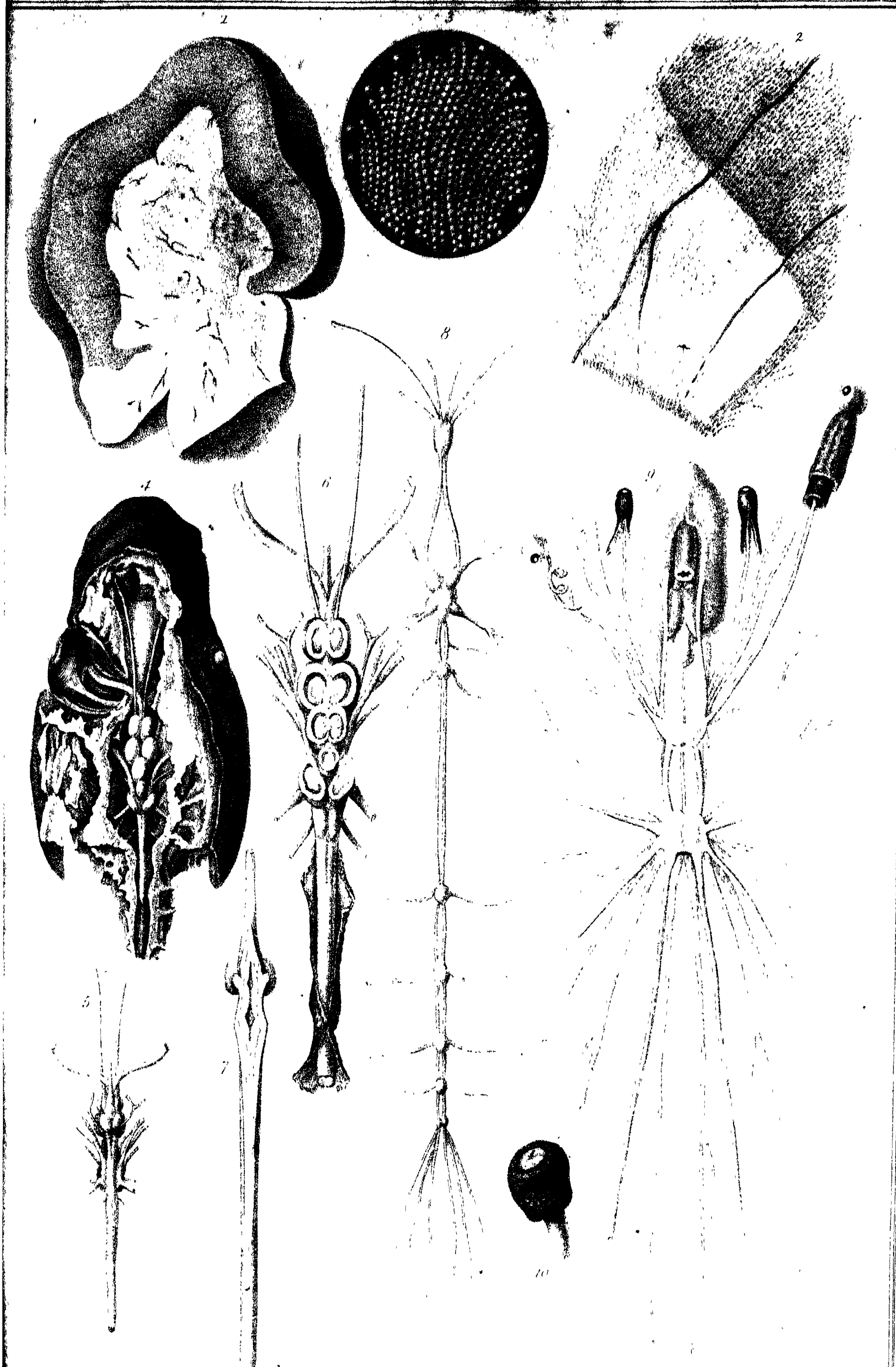
Fig. 2. The same parts in a large caterpillar; magnified four diameters.

Fig. 3. The same parts in the lobster; natural size.

Fig. 4. The same parts in the earth worm; magnified two diameters.

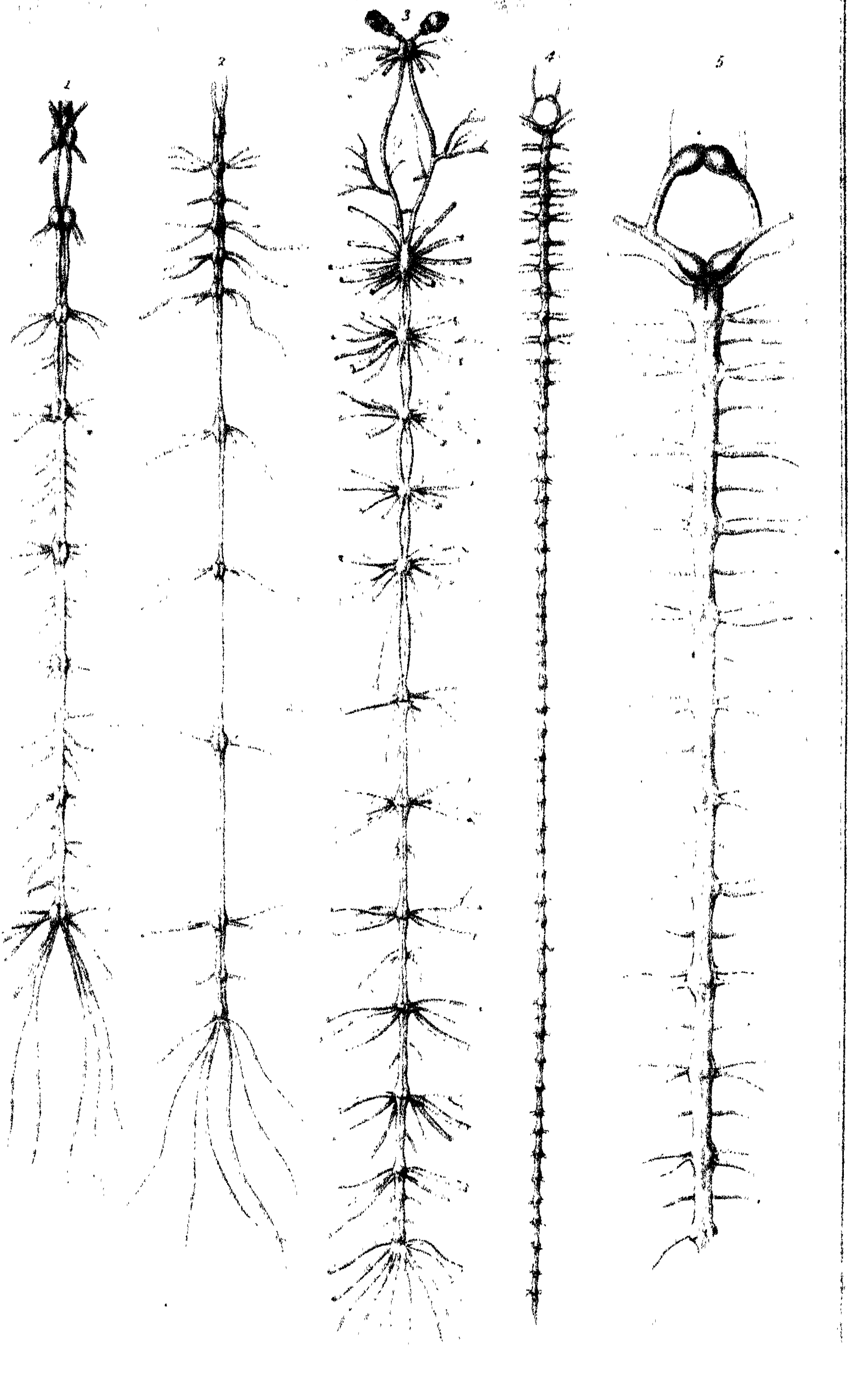
Fig. 5. The upper part of the same earth worm; magnified eight diameters.

In Fig. 1, 2, 4, and 5, the ganglions, as well as the brain and spinal marrow, are upon too small a scale to admit of accurate examination, but in a large lobster, the distinction I have made between the brain and spinal marrow and the ganglions, was satisfactorily confirmed, and even in the earth worm it was sufficiently distinct.



Franz Bauer delt.

J. Basire sculpt.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II. *Some observations on the Migration of Birds. By the late* Edward Jenner, *M. D. F. R. S.; with an Introductory Letter to Sir* Humphry Davy, *Bart. Pres. R. S. By the Rev. G. C.* Jenner.

Read November 27, 1823.

Sir,

It had long been the intention of my late revered Uncle, Dr. Jenner, to lay the accompanying observations on the Migration of Birds, before the Royal Society, as well from inclination, as to redeem a pledge he had given some years ago to that learned Body; but which he was unable to accomplish, in consequence of his extensive correspondence with almost every part of the globe on the interesting subject of Vaccination, which occupied nearly the whole of the time his more immediate professional avocations would allow him to bestow on other objects.

It was my peculiar happiness to accompany Dr. Jenner in most of his investigations of the phenomena of migration; and the Paper I have now the honour of presenting, was left in my hands at the time of his decease.

Had it pleased Providence to have spared him a little longer, he might, probably, have corrected some inaccuracies in the style and order of his Paper, that may now, perhaps,

appear conspicuous to the reader, but which I did not conceive myself justified in attempting.

I have the honour to be, Sir,

Your most obedient humble servant,

Stone, near Berkeley,
May 29, 1823.

G. C. JENNER.

To Sir HUMPHRY DAVY, Bart.
President of the Royal Society,
&c. &c. &c.

I.

IT is not my intention, in the following pages, to give a general history of the migration of birds. The order in which they appear and disappear, their respective habits, and many other observations, have been given with considerable accuracy by several naturalists, who have paid attention to this very curious subject. It is with a view of representing some facts, hitherto unnoticed, chiefly with respect to the *cause*, which excites the bird, at certain seasons of the year, to quit one country for another, that I communicate the following pages to this learned body.

But before I proceed to state my observations on this head, it may be necessary to adduce some arguments first, in support of the reality of migration, the fact itself not being generally admitted; and, secondly, against the hypothesis of a state of torpor, or what has been called the hibernating system.

In the first place, the ability of birds to take immensely long flights, is proved by the observations of almost every person conversant with the seas. To the many instances already recorded, I shall add the following:

My late nephew, Lieutenant JENNER, on his passage to

Newfoundland, saw, on the 20th of May, the hobby hawk. It came on board, and was secured The day following a swallow came on board. At this time the ship was steering a course direct for that island, and was not within the distance of an hundred leagues of any land. His brother, the Reverend G. C. JENNER, in crossing the Atlantic, observed an owl (of what species he could not precisely ascertain, but he believes it to be the common brown owl) gliding over the ocean with as much apparent ease as if it had been seeking for a mouse among its native fields.* Wild geese have frequently been shot in Newfoundland, whose crops were plentifully stored with maize, or indian corn: consequently, these birds must have taken a pretty bold flight in a short space of time, as no corn of this kind is cultivated within a vast distance of that island. These, however, I do not consider as migrations of any farther consequence, than just to show the powers of the wing.

My ingenious friend and neighbour, the late Reverend NATHANIEL THORNBURY, who had occasionally visited Holland, informed me, that the pigeons about the Hague make a daily marauding excursion, at certain seasons, to the opposite shore of Norfolk, to feed on vetches, a distance of forty leagues. Now, may not this be almost considered as daring a flight as that of the bird which crosses the Atlantic? For it

* Mr. JENNER informs me, that in subsequent voyages he has taken, in the Atlantic, several hundred miles from land, the nuthatch, hoopoe, and snipe; and has often seen small birds of the linnet kind. Of the latter, a large flock came on board, perched on the rigging, appeared very lively, and after adjusting their plumage, and chirping in concert for a few minutes, took their flight in a direction for the Azores.

is not at all probable that the shores of this country can be visible to the flock when they set out.

Again. Is there not something as extraordinary in the pigeon, which can, in a few hours, find out its home, though taken away in a box and totally excluded from the light, to the distance of two hundred miles, as in that bird which quits one shore to seek another, whatever may be the extent of intervening seas? The fact seems to be, that we, the *little lords of the creation*, are too prone to measure the sentient principle in animals by the scale of our own ideas, and thus, unwillingly, allow them to possess faculties which may surpass our own, though peculiarly appropriate to their respective natures; but a little reflection must compel us to confess, that they are endowed with discriminating powers totally unknown to, and for ever unattainable by man. I have no objection to admit the possibility that birds may be overtaken by the cold of winter, and thus be thrown into the situation of other animals which remain torpid at that season; though I must own I never witnessed the fact, nor could I ever obtain evidence on the subject that was to me satisfactory: but as it has been often asserted, may I be allowed to suppose, that some deception might have been practised with the design of misleading those to whom it might seem to have appeared obvious? For far be it from me to insinuate that the subject has been wilfully misrepresented by those naturalists, who have stated it as a fact. Yet how careful should we be in the investigation of all subjects in natural history, which may captivate, by their apparent novelty!

If birds crept into holes and crevices to hibernate, would they not, like quadrupeds, creep out again in a languid state,

their fat all absorbed, and their bodies emaciated? We see this fact exemplified in the hedge-hog, one of the most remarkable of our hibernating animals, which retires to its hut at the approach of winter, with vast stores of fat placed in every situation where nature could find room for it. This fat is its only source of nutrition for the winter, which, by the time the sun rouses it to fresh life and activity, is exhausted, and the animal comes forth thin and emaciated. But the case with birds is extremely different. If, on the first day of its appearance, a martin, a swift, or a redstart be examined, it will be found as plump and fleshy as at any season during its stay; it appears also as strong on the wing, and as full of activity at that period as at any other during its abode with us. How the cuckoo, that disappears at so early and so hot a season as the first week in July, can become torpid, is beyond the power of conception.

The apparent incapability of the landrail to perform the task of migration, has often been so strongly adduced as a presumptive argument in favour of the hibernating system, that those who do not admit that of migration, were it to remain unnoticed, might urge it as an objection. It must be admitted, that a superficial examination of the habits of this bird, tends to favour the supposition of its incapacity for so great an exploit, as it often rises from the ground like an half animated lump, and seems, with difficulty, to take a flight of a hundred yards; but let us remark its powers when seriously alarmed. Should it be forced upon the wing by any extraordinary cause, by the pursuit of a hawk, for example, the velocity of its flight, and the rapidity of its evolutions to avoid the common enemy of its race, will at once appear.

This is no very rare exhibition. Necessity here, as in migration, becomes the parent of exertion, which when thus called forth, cannot be shown in a much greater degree by any of the feathered tribe. The moor-hen (which winters with us) gives another instance of what a bird, which appears so much to want activity in its ordinary flights, is capable of performing when exertion is actually required. When pursued by a hawk, and self preservation calls up all its powers, it may be seen to rush up into the air with amazing velocity, almost as high as the eye can reach, then darting down with an equal pace, it often, by such rapid manœuvres, escapes the destructive talons of its swift pursuer.

It is a remarkable fact that the swallow tribe, and probably many other birds which absent themselves at stated periods, should return annually to the same spot to build their nests. The swift, which for nine months has some distant region to roam in, was selected for the purpose of an experiment to ascertain this with precision. At a farm-house in this neighbourhood I procured several swifts, and by taking off two claws from the foot of twelve, I fixed upon them an indelible mark. The year following their nesting places were examined in an evening when they had retired to roost, and there I found several of the marked birds. The second and third year a similar search was made, and did not fail to produce some of those which were marked. I now ceased to make an annual search, but at the expiration of seven years, a cat was seen to bring a bird into the farmer's kitchen, and this also proved to be one of those marked for the experiment.

That the bird, when the stimulus for migration is given,

with the choice before it of almost any part of Europe for its annual excursion, should so uniformly not only revisit this island, but even select the same spot for its breeding place, is certainly a wonderful occurrence. But if birds were not instinctively directed to return to their old haunts, should we not find them over crowding some situations, while others would be left desolate? And would not this be the case if the search of food was the object of their migration? However it may be admissible, in one point of view, to consider the bird in its state of migration from this country, as a nearer neighbour than at first might be conceived, if we may be allowed to consider distance, or space, in the instance before us, as governed by the power of progressive motion, of what consequence is it to the swift, which, to use the animated expression of Mr. WHITE, "dashes through the air with the "inconceivable swiftness of a meteor," whether he comes to us from some neighbouring country, or the shores of Africa? The wonder excited by the return of these birds again to their old nesting places, would at once cease, if we could believe what has been asserted by some naturalists, and gained credit with many, namely, that at the time they disappear from us, they submerse themselves in ponds and rivers, and in this situation become torpid. If this idea had not been encouraged and supported by some new hypothesis, I should hardly have thought it necessary to have taken any serious notice of it; but as the matter now stands, I will just state my opinion, why I think it impossible for any birds to be disposed of in this way.

Permit me first to call to your recollection the season of the year at which many of these birds disappear. It happens

when they feel no cold blast to benumb them, and when the common food with which they are supported, is distributed through the air in the greatest abundance. At such a time, what can be the inducement to them and their young ones, which have but just began to enjoy the motion of their wings, and play among the sun-beams, to take this dreary plunge? And how is the office of respiration to be performed during the nine months watery residence? The structure of the lungs of birds, differs not essentially from that of quadrupeds, and therefore all communication with the atmosphere being cut off from the first moment of submersion, the possibility of a bird living nine months, or indeed as many minutes under water, appears to be totally irreconcilable with the nature of their structure. I have taken a swift about the 10th of August, which may be considered as the eve of its departure, and plunged it into water; but like the generality of animals which respire atmospheric air, it was dead in two minutes.

The late Doctor BEDDOES has thrown out a supposition, that by frequent immersion in water, the association between the movements of the heart and lungs might perhaps be destroyed, and that an animal might be inured to live commodiously for any time under water. As this will probably give new vigour to the languid system of the advocates for the submersion of birds, I think it incumbent upon me to mention it.

Though we frequently see the swallow and the martin sprinkle and splash themselves as they glide over the surfaces of ponds and rivers, yet we never see them dip under for a single moment; indeed a few plunges would so moisten

their wings as to prevent their flying, and we should see them occasionally in this disordered state fluttering on the shore. If they went to the sea side, and got beyond the reach of the eye to inure themselves to this element, how could they return, divested as they must be either of the means of swimming or flying? Whoever has observed the common tame duck driven to the necessity of repeatedly diving from the pursuit of a water-dog, must have noticed how exhausted it rises to the surface of the water after a short period of submersion, and how incapable it is of flying, in consequence of the soaking of its wings. The same may be said of birds more in the habit of diving, the grebes and divers. When entangled in a net they soon perish, or when they happen to dive under ice that may chance to overspread a pond; no uncommon place of resort for some of the smaller species of grebes.

I have always been much attached to that faithful animal, the Newfoundland dog, and have often procured from that country those dogs that had been much accustomed to diving, and which had been kept to the practice; yet I never observed that any of them attained by habit the power of remaining under water longer than thirty seconds, and even then, on rising to the surface, they appeared confused. Negroes and other men who have been employed in seeking among sunken rocks the hidden treasures of the deep, are said to have acquired a habit of remaining some minutes under water, but the time was probably measured by a rude guess, and not by a stop watch.

Having thus called the attention of the Society to such statements as give support to the fact of migration, and

having also endeavoured to controvert the notion of an hibernating system, I beg to draw their attention to what I conceive to be the true *cause* of migration.

At the coming on of spring we observe our more domestic birds, those that approach our houses, and are most familiar to us, assuming new habits. The voice, gesticulation, and the attachment which the male begins to show to the female, plainly indicate some new agency acting upon the constitution.* This newly excited influence, which so conspicuously alters the habits of our birds at home, is, at the same time, exerting itself abroad upon those which are destined to resort hither. *It is the preparation which nature is making for the production of an offspring by a new arrangement in the structure of the sexual organs, (viz.) the enlargement of the testes in the male, and the ovaria in the female.*

No sooner is the impulse arising from this change sufficiently felt, than the birds are directed to seek a country where they can for a while be better accommodated with succours for their infant brood, than in that from which they depart.†

* The Rook, among many others, exhibits a familiar instance of the change of voice.

† Birds of the same species that are commonly stationary in this island throughout the year (I say *commonly*, for all, I believe, occasionally migrate), are migrators in other countries. The adult bird might, perhaps, find a subsistence for itself in the country it quits during the incubating season, but the nestling is probably the object nature chiefly holds in view, both with respect to food, and to the temperature of the air in which it is first to feel existence The one may be unfit or too scanty, and the other too hot or too cold. It is wonderful to see with what peculiar care the parent birds select the food for their young until they are four or five days old. For the most part it is purely animal, but not an atom even of that is suffered to go into the nestling's stomach, that is not perfectly adapted to the tender state of

It is not at the commencement of this enlargement, nor until it is considerably advanced, that the birds are prompted to migrate; and this is very wisely ordered; for were they to set off, when first the testes and ovaria begin to grow tumid, they must waste much time here unnecessarily, and indeed arrive at too early a period to find a supply of food. Very little time is lost after their arrival, before they form their connubial alliances.* The business of nesting then begins; and as a convincing proof that nesting is the chief cause of their errand here, this, and its natural consequences, occupy their attention from the time of their coming to the day of their departure. This is illustrated by the dispatch which some of them make in performing the object of their mission. The cuckoo

its digestive powers. While the swift is feeding on small beetles that have hard crustaceous wings, and whose habitations are the air, its nestlings are fed in their early state with gnats. The sparrow, a granivorous bird, feeds its young for several days after they are hatched, with the softest insects only, now and then introducing a little coarse sand, smooth on the surface, to inure the stomach, as I suppose, to bear the same kind of substances in a more rugged state, which will shortly be required.

* Should a fatal accident befal either the male or female bird after this alliance is newly formed, no time is lost in unavailing sorrow, nor any great nicety shown in forming a new connection, as the following little history will evince. A pair of magpies began to build their nest in a gentleman's garden at Burbage, in Wiltshire. Disliking their familiarity, he shot one of them from an ambush made for the purpose. The next day there were again a pair going on with the work. One of these was also shot. The loss was not long in repairing, for the day following the pair were again complete, when another fell a victim to the gun. Thus the gentleman went on destroying one of them daily until he had killed seven; but all to no purpose—the remaining magpie soon found another mate. The nest was finished, and young ones were produced, which were suffered to fly. This is an extraordinary fact.—It seems to show that nature has a reserve of birds in an unconnected state, immediately ready to repair losses. Were the whole to pair at once, the circumjacent country might be insufficient to furnish food for the immense number of young ones that must burst forth at the same time.

finishes this business in a shorter space of time than any other bird, but as he deviates so widely from the common laws of the feathered society, I shall select the swift, as a better example for pointing out the fact. The swift shows himself here about the beginning of May (sometimes a few stragglers appear earlier) and by the beginning of August he has completely reared his young ones, which seldom consist of more than two. At once the old birds and their family take their leave and are seen no more for that season. Now his farther residence cannot be rendered unpleasant by any disagreeable change in the temperature of the air, or from a scarcity of his common food, which at this time abounds in the greatest plenty. This circumstance of the early departure of the swift, without a more apparent cause, seems to have excited much astonishment and perplexity in the mind of that attentive and ingenious naturalist, the late Mr. WHITE, of Selborne. Speaking of the swift, (Letter XXI. page 184), he says, " But in nothing are swifts more singular than in " their early retreat. They retire, as to the main body of " them, by the tenth of August, and sometimes a few days " sooner; and every straggler invariably withdraws by the " twentieth, while their congeners all of them stay till the " beginning of October, many of them all through that " month, and some occasionally to the beginning of Novem-" ber. This early retreat is mysterious and wonderful, since " that time is often the sweetest season of the year. But " what is more extraordinary, they begin to retire still ear-" lier in the most southerly parts of Andalusia, where they " can be no ways influenced by any defect of heat; or as " one might suppose, defect of food. Are they regulated in

"their motions with us by a failure of food, or by a propen-
"sity to moulting, or by a disposition to rest after so rapid
"a life, or by what? This is one of those incidents in Natural
"History that not only baffles our searches, but almost
eludes our guesses!" Thus Mr. WHITE.

Now, should the principle I have laid down be admitted, namely, that these birds come here for scarcely any other purpose than to produce an offspring, and retreat when the task is finished, how easily will all circumstances be reconciled? and how little mysterious will those things appear which naturally seemed unaccountable, not only to the amiable author from whom the foregoing passage is taken, but also to others, who have written before on the same subject.

It is somewhat remarkable, that so sagacious a philosopher as the illustrious and learned RAY, who so clearly saw the object of migration in fishes, should not also have been led to a sight of it in birds. After making a very just observation respecting salmons, that quit the sea and ascend up rivers with no other view than to find a place of security for their spawn in the sand; he directly says again, adverting to birds, "What moves them to shift their quarters? You will
"say the disagreeableness of the air to the constitution of
"their bodies, or want of food."*

The spring migrating birds do not arrive here at first in very large numbers. It may be observed, that in the early part of April a few swallows may be seen; soon after these a few solitary martins, and as the month advances now and then a swift. On the walls of Berkeley Castle, martins build their nests in great numbers. I availed myself of their situ-

* RAY, on the Wisdom of God in the Creation. Part 1., page 128.

ation, and took several of them on the same night, the latter end of May. On dissection, the cause of their gradual and successive migrations appeared obvious, the testes and ovaria being in very different states of progressive forwardness. While one bird presented embryo eggs in the ovarium as large as peas, in another they were found no larger than hemp-seed. These were the extremes; for in the other birds there appeared all the intermediate stages, from the enlargement of the ovaria, sufficient to give the stimulus for migration, to the degree of forwardness just described. The same gradations in the state of the testes of the male corresponded with that of the ovaria in the females. This progressive arrival is not confined to the swallow tribe: all the birds that come early in the spring appear in the same gradual manner. I cannot help observing, that here the wise design of Providence is very conspicuous. Their appearance keeps pace with that of the insects which are to afford them food. If the numbers which flock in upon us in May, were to arrive in April, when only part of them appear, all must be insufficiently supplied, and many of course perish from a want of the needful succours; but by the middle of May, myriads of insects have produced eggs, and great numbers have either brought forth, or matured their progeny; and it may be remarked, there is still a greater increase of insect food by the time the young birds begin to require it. Swallows, on their first coming, feed principally upon gnats. These insects are called forth from their wintry retreats when the air is but moderately heated, 48 degrees of FAHRENHEIT's thermometer being sufficient to put them on the wing. It is in pursuit of them that we see, in cool weather, the

swallow incessantly skimming over the surface of ponds and brooks; and their thus early hovering over water has strengthened the idea of their having lately emerged from their watery abode, where they are supposed to have lain dormant during the winter. But they are driven by necessity to feed on the gnat. Like the swift and martin, their more favourite food is a small beetle of the scarabæus kind, which, on dissection, I have found in far greater abundance in their stomachs than any other insects.

The tumid state of the testes and ovaria sometimes comes on prematurely, and in the same manner sometimes subsides.* When this happens, swallows and martins desert their nestlings, and leave them to perish in the nest. The economy of the animal seems to be regulated by some external impulse, which leads to a train of consequences. When this change in the testes and ovaria takes place, the bird becomes impelled by a stronger principle, that is, the desire of self preservation. This sometimes happens when they produce a very late hatch. A pair of martins hatched four broods of young ones in the house of a tradesman in this place in the year 1786. The latter brood was hatched in the early part of October. About the middle of the month the old birds went off, and left their young ones, about half fledged, to perish. The pair returned to the nest the 17th of May, 1787, and threw the skeletons out.

Thus scarcely a winter passes but we hear of a nest of robins, hedge-sparrows, and some others of the smaller birds. We have been informed by PENNANT, and it has been noticed also by others, that the cuckoo has been heard to give his song so early as the middle of February, two months sooner

than the usual time. The same deviation from the ordinary course of nature, which prematurely occasions the pairing of our domestic birds above-mentioned, proves the stimulus, I conceive, to certain unseasonable migrations, and accounts for the irregularity first noticed. The same argument is of course applicable to the premature appearance of any other migrating birds. The month of March sometimes affords us warm weather for several successive days. At this time I have often seen the snake basking under a hedge. The lizard too, has been invited from his cold retreat; but never could I see the swallow or the martin, although I have taken every opportunity of looking for them during the transient sunshine, and made diligent enquiries of others. At the further advancement of spring, often in April, when, from the long prevalence of north-easterly winds, the weather becomes unseasonably cold, and even frosty, swallows, martins, and other early migrators appear among us. But they soon experience the hardships of an inhospitable reception: the insects that should afford them food being still in a state of torpor in their wintry recesses, and unless called forth by some agreeable change in the air, the unfortunate birds perish for want of food. This I have known happen during an inclement spring, and have picked up starved martins under their nesting places, and willow wrens, which have perished under hedges, through a want of succours.

Unlike the migrating birds that winter with us, of which I shall speak in a subsequent part of this paper, the spring or summer birds do not possess the disposition to change the scene and seek a more genial clime, when this country is so overspread by frost as to deny them their common supplies.

This, I imagine, will admit of an easy explanation. The winter birds require nothing here but food and shelter. Our summer visitors come for more various and important purposes. Had they, like the former birds, been endowed with a disposition to wander on certain changes of the atmosphere, the great design of their migration, as it must have proved fatal to the business of incubation and the rearing of their young, would have been frustrated. It may be worthy of remark, that both the summer and winter migrating birds are, on their arrival here, well received by the domestic natives, and neither create quarrels nor excite fears. The redstart builds its nest in the same tree with the titmouse, and the redwing feeds peaceably in the same meadow with the starling.

I proceed now to make some observations on another kind of migration, directly opposite to the foregoing, namely, the return of the spring migrators to their respective *homes*.

The great disproportion in numbers between those species of birds which quit the country in summer, and those that leave it at the autumnal season, has led naturalists to lose sight of the early migrators, and to confine their reflections on the subject to the late ones only. Hence the common observation, that they are *all* driven off through a failure of food, or a cold temperature of the air. But seeing that many of them disappear in the summer season, when food is placed before them in the greatest plenty, we must seek for some other cause. If we examine what is now going forward in the animal economy, dissection will point out a change in the testes and ovaria, the very opposite to that which took place

in the spring. These parts now begin to shrink,* the disposition for raising a farther progeny ceases, and the nuptial knot is dissolved. What inducement have they to stay longer in that country where, I think, it clearly appears their chief object is to multiply their species? This being now effected, they retire to different parts of the globe, doubtless better suited to their general dispositions and wants, when disengaged from parental duties. In many of the migrating species, indeed in the far greater number, the disposition for farther incubation, and the season for their procuring a farther supply of insect food, cease at the same time. It is pretty evident from the habits of the cuckoo and the swift, that quit us in the summer as soon as their nesting is at an end, that swallows, martins, and those birds that disappear in the autumn, would depart at an earlier season, even though their supplies were to continue, if the rearing of their young were perfected. Indeed, as has been before observed, so strong does this propensity now and then appear, that it overcomes even the obligation of rearing their young when hatched late in the season, and they are sometimes left in a callow state to perish in their nests. This premature departure, probably arises from a reverse of that stimulus which occasions the too early migration of the spring birds, as has been noticed in a former part of this essay, namely, a change which takes place in organization.

One of the most singular occurrences in the history of migration, is the mode of departure of the young birds from the country where they were produced. It may be conceived

* I examined a female cuckoo the first week in July, and found the oviduct shrivelled, and all the eggs disposed of.

that the bird which had once crossed the Atlantic, or any other ocean, might have something impressed upon it that should prove an inducement to its return; but this cannot be an incitement to the young one. The identical bird, which but a few weeks before burst from the shell, now unerringly finds, without any apparent guide, a track that leads it safely to the place of its destination, perhaps in many instances over the widest oceans.

It is well known, that those birds which incubate several times in the course of one summer, forsake their first broods when they no longer require their protection: and being now alienated, they cannot, in their parents, find the guides that conduct their course. As swallows and martins congregate* prior to their departure from us, it may be said that their young, though discarded, may mingle with the common flock, and in this particular instance I am ready to admit that it is probable they may do so; but there are many migrating birds that never either associate with swallows and martins, or join together in flocks, as the nightingale, redstart, and indeed the far greater number. As a striking proof that the parent bird cannot possibly be the guide, in one instance at least, we may point out the cuckoo, whose offspring finds a distant shore in perfect safety, although it could never know the parent to whom it was indebted for existence, and though

* Swallows and martins congregate on the sunny sides of buildings for the sake of warmth, and not, as it is generally supposed, to hold a kind of consultation previous to their final departure. In the wet summer of 1821, when the air was unusually chilled by the long continued rains, they were observed to assemble, during some intervals of sunshine, for several successive mornings, as early as the middle of July; and in the present year (1822), I remarked the same on some mornings that were unseasonably cold about the middle of August.

its existence in numberless instances must have taken place even after the departure of the parent. For the old cuckoos invariably leave us early in July, when many of their eggs are yet unhatched in the nests of those small birds to whose fostering care they are entrusted. Compared with quadrupeds, and some other animals, birds may be considered as acquiring the adult state at an early period, and the young bird, at the time of its leaving us, may be looked upon as possessing power equal to the old one in procuring food, velocity of flight, &c. The parent bird, from having lost that stimulus by the subsiding of the testes and ovaria, which urged it to incubation and detained it here, is now reduced to a condition similar to that of its offspring, both falling into the same habits, and remaining in the same state with respect to organization, until the returning calls of nature urge them to quit that country again to which they are *now* about to depart.

II.

Winter Birds of Passage.

"We have, 'tis hoped, made it pretty evident that summer "birds of passage come to and depart from us at certain sea-"sons of the year, merely for the sake of a more agreeable "degree of warmth, and a greater plenty of food; both which "advantages they procure by an alternate change of climate; "but the migration of winter birds of passage, and particu-"larly of fieldfares and redwings, is much more difficult to "be accounted for, there being no such apparent necessity "either on the score of food or climate, for their departure "from us."

Mr. CATESBY, Phil. Trans. No. 483.

The winter birds of passage, as they are commonly called, begin to take their leave of us about the same time that the spring migrators are taking wing to pay us their annual visit. As the latter appear among us in gradual succession, so in like manner the former disappear. They are both actuated by the same impulse, the former in leaving, and the latter in coming to this country, namely, the enlarged state of the testes and ovarium. As soon as the stimulus becomes sufficiently felt, they quit their homes in quest of a country better suited to their intended purpose than their own.

That a want of food cannot be the inducement, must be obvious to the slightest observer. When the redwing and fieldfare quit this country, it abounds with that food which they prefer to any other; and at this time they are in the finest condition; the redwings often enjoying their plenty by assembling together on trees, and there uniting their feeble voices, make no unpleasant song.*

The winter birds (the females at least) may be said to seek a better accommodation, upon the same principle as the poor woman who quits her cottage for the comforts of a Lying-in Hospital. Here, both herself and suckling are for a while supported in that peculiar way which their situations at that time require. For this reason, conceiving it will tend to lessen confusion, I choose to call this country the *home* of the winter birds (though not natives), and the countries from whence they come, the home of the summer birds, looking

* The same thing happens through the winter, whenever the weather has long continued so mild as to allow them plenty of insect food. The starling, and some other birds which have a short note and weak voice, unites with its companions in the spring, and forms a similar concert.

upon the latter merely as visitors; and let it be recollected how soon the visits of some of them are paid; for being governed by an unerring principle, they stay to accomplish one great design only, that of rearing their young, and then return.

The countries to which many of the winter birds retire not being very far distant, are better known to us than those to which the summer birds migrate; but I must forbear entering into an enquiry upon this subject, as remote from the design of this paper; and indeed it may be thought I have already, in some instances, digressed too widely from my original purpose.

The migration of the winter birds is less distinctly marked than that of the spring migrators. The snipe, the wild-duck, the wood-pigeon, breed here in considerable numbers; the two latter indeed, particularly the wood-pigeon, are so numerous in summer, that we should hardly be reminded of the migration, did they not pour in upon us in such immense flocks in the winter. They are accompanied by the stock-dove, which I have never known to breed here. The home-bred wild-ducks are easily distinguished by the men who attend decoy-pools, by the meanness of their plumage, when compared to the brightness of those birds which come from abroad. The former are taken some weeks earlier than the latter.

The most conspicuous among the winter migrating birds are the redwings and fieldfares. These are regular and uniform in their appearance and disappearance, and I believe never risk the trial of incubation here, at least I never could hear of a single instance. The food of these birds has in the

works of every naturalist I have ever had access to, who had written on the subject, been pointed out as the haw, the fruit of the white thorn.*

This is an error that has long wanted a correction, for in open weather they take them in very scanty quantities, and feed on the ground on worms and such insects as they can find. Although repeated examinations of the contents of the stomach have afforded the best proof of this, yet there is scarcely any need of calling in its aid in the present instance, as we may be convinced of the fact, by seeing them in flocks feeding on the ground in open fields and meadows. I do not deny their taking the haw and other vegetable food from the hedges, but they do it in so sparing a way, that I have remarked, that red wings and fieldfares die through hunger during the long continuance of frosty weather, while the haws on the hedges were by no means deficient. The occasional departure of these and some other winter birds during a long continued frost, must be very obvious. The greater number disappear soon after its commencement, if it sets in very severely: some few are always left behind and are soon starved, if not fortunately relieved by a thaw. Those that are driven to this necessitous migration, probably pursue a track that quickly leads them out of the reach of frost. Of these flights I shall produce instances, which render it probable that they are able even to out-strip its course.

The approach of intense frost is often to a certainty made known to us by the appearance of a numerous tribe of water-

* "The principal food of these birds while with us, is the fruit of the white "thorn, or haws, which hang on our hedges in winter in prodigious plenty."

Phil. Trans. Vol. XLIV. p. 435.

birds, some of which are rare, and seldom show themselves here on any other occasion. We commonly see them three or four days prior to the setting in of very severe frosty weather. This was manifest at the latter end of the year 1794, at the coming on of the severe season that ensued. In the river Severn, about a mile and a half to the westward of this place, were seen and taken many species of water-birds, that generally confine themselves to the more northern regions. Far more pleasant is it to see during the continuance of hard frost, the return of those birds which had left us at the beginning. These are pleasant omens, and most certainly forebode a thaw. The following example shows how soon they catch the first opportunity of again seeking those countries from which they were so lately driven by necessity. The day preceding the thaw, the frost being then intense, a gentleman who was shooting observed a large flock of fieldfares, birds that are extremely common here in milder weather. They were as much untamed as if no frost had appeared in our island. He had the good fortune to shoot one of them, which was brought to me. I found it as fat and plump, and in every respect in as good condition, as if it had remained here undisturbed, and had found provision in the greatest plenty, though it was without a particle of food in its stomach. Its last meal was digested; and the frost still remaining, it could find no food for its present support. Now it is very obvious that this bird, and its companions, must have taken a long flight, and probably in a very short space of time; for the intense frost, that was of such duration and so severely felt here, extended far into the more southern parts of Europe, beyond which they must have resorted for that

plenty of food which gave plumpness to the one I examined, and doubtless to the whole flock, from their appearing so wild and vigorous. It clearly appears, that in their flight they exceeded the progress of the thaw, as the northern birds did that of the frost. This thaw, though it was again succeeded by frost, came on very rapidly, and occasioned, by the sudden melting of the snow, those destructive inundations through the kingdom, that will not readily be forgotten.

This account of the fieldfare sets the fact of migration, though from an accidental cause, beyond the reach of doubt. There was no support for it here: the ground was deeply covered with snow, and the intense frost, by its long duration, had destroyed every thing that could afford it succour; it must, therefore, have taken a long flight from this country, and returned to it again at the approach of temperate weather.

Having already made so many digressions, I cannot add another without offering an apology; but as there is something so like providential design in the order in which the song birds chaunt out their warblings during a long summers day, I trust the Society will pardon my laying before them the following observations on the subject.

We must observe, that nature never gives one property *only*, to the same individual substance. Through every gradation from the clod we tread upon to the glorious sun which animates the whole terrestrial system, we may find a vast variety of purposes for which the same body was created. If we look on the simplest vegetable, or the reptile it supports, how various, yet how important in the economy of nature, are the offices they are intended to perform! The

bird, I have said, is directed to this island at a certain season of the year to produce and rear its young. This appears to be the grand intention which nature has in view; but in consequence of the observation just made, its presence here may answer many secondary purposes; among these I shall notice the following. The beneficent Author of nature seems to spare no pains in cheering the heart of man with every thing that is delightful in the summer season. We may be indulged with the company of these visitors, perhaps, to heighten, by the novelty of their appearance, and pleasing variety of their notes, the native scenes. How sweetly, at the return of spring, do the notes of the cuckoo first burst upon the ear; and what apathy must that soul possess, that does not feel a soft emotion at the song of the nightingale, (surely it must be "fit for treasons, stratagems, and spoils") and how wisely is it contrived that a general stillness should prevail while this heavenly bird is pouring forth its plaintive and melodious strains,—strains that so sweetly accord with the evening hour!—Some of our foreign visitors, it may be said, are inharmonious minstrels, and rather disturb than aid the general concert. In the midst of a soft warm summer's day, when the martin is gently floating on the air, not only pleasing us with the peculiar delicacy of its note, but with the elegance of its meandering; when the blackcap is vying with the goldfinch, and the linnet with the woodlark, a dozen swifts rush from some neighbouring battlement, and set up a most discordant screaming. Yet all is perfect. The interruption is of short duration, and without it, the long continued warbling of the softer singing birds would pall and tire the listening ear with excess of melody, as the exhila-

rating beams of the sun, were they not at intervals intercepted by clouds, would rob the heart of the gaiety they for a while inspire, and sink it into languor. There is a perfect consistency in the order in which nature seems to have directed the singing birds to fill up the day with their pleasing harmony. To an observer of those divine laws which harmonize the general order of things, there appears a design in the arrangement of this sylvan minstrelsy. It is not in the haunted meadow, nor frequented field, we are to expect the gratification of indulging ourselves in this pleasing speculation to its full extent; we must seek for it in the park, the forest, or some sequestered dell, half enclosed by the coppice or the wood.

First the robin, and not the lark, as has been generally imagined, as soon as twilight has drawn the imperceptible line between night and day, begins his lonely song. How sweetly does this harmonize with the soft dawning of day! He goes on till the twinkling sun-beams begin to tell him his notes no longer accord with the rising scene. Up starts the lark, and with him a variety of sprightly songsters, whose lively notes are in perfect correspondence with the gaiety of the morning. The general warbling continues, with now and then an interruption, for reasons before assigned, by the transient croak of the raven, the screaming of the jay and the swift, or the pert chattering of the daw. The nightingale, unwearied by the vocal exertions of the night, withdraws not proudly by day from his inferiors in song, but joins them in the general harmony. The thrush is wisely placed on the summit of some lofty tree, that its loud and piercing notes may be softened by distance before they reach the ear, while

the mellow black-bird seeks the inferior branches. Should the sun, having been eclipsed with a cloud, shine forth with fresh effulgence, how frequently we see the goldfinch perch on some blossomed bough, and hear his song poured forth in a strain peculiarly energetic; much more sonorous and lively now than at any other time; while the sun, full shining on his beautiful plumes, displays his golden wings and crimson crest to charming advantage. The notes of the cuckoo blend with this cheering concert in a perfectly pleasing manner, and, for a short time, are highly grateful to the ear; but, sweet as this singular song is, it would tire by its uniformity, were it not given in so transient a manner. At length, evening advances—the performers gradually retire, and the concert softly dies away. The sun is seen no more. The robin again sets up his twilight song, till the still more serene hour of night sends him to the bower to rest. And now to close the scene in full and perfect harmony, no sooner is the voice of the robin hushed, and night again spreads a gloom over the horizon, than the owl sends forth his slow and solemn tones. They are more than plaintive, and less than melancholy, and tend to inspire the imagination with a train of contemplations well adapted to the serious hour. Thus we see that birds, the subject of my present enquiry, bear no inconsiderable share in harmonizing some of the most beautiful and interesting scenes in nature.

But let me here remark—how ill would the singing of birds agree with the general appearance of winter—the leafless tree,—the snowy mead,—the frozen rivulet! Yet it must be noticed here, that these rigors, in the midst of this dreary season, are sometimes suddenly softened, and a temperate

state of the air succeeds. We are then so enlivened by the transition from extreme cold, to a temperature comparatively warm, that we can listen with pleasure to the enfeebled notes of some of the song birds. How admirable the contrivance! There are several birds which have no continued flow of notes, but a kind of chirp only, consisting of some variety of sounds. During a long continued frost, the earth affords many of the feathered tribe so scanty an allowance, that they preserve themselves with difficulty from perishing; a sudden thaw takes place,—plenty at once appears, and every crop is filled. Tis then we see the redwing and starling assemble in large flocks among elms and apple trees, and, by uniting their voices, produce a song not in the least discordant, but, on the contrary, extremely harmonious. At this time the thrush, and even the blackbird, will occasionally afford us a transient song; but it may be observed, that the notes of these birds are rather to be considered as plaintive, than lively. The lark, too, will sometimes mount in the air, beguiled, as it were, by the faint rays of a wintery sun, but his notes are then as poor and feeble as the beams that call him forth. The robin indeed cheers us with his song during the whole of the winter, unless driven off by intense frost, and is the only bird I know, whose notes, at this time, would fully accord with our feelings, so perfectly do they mingle with the surrounding order of things. The goldfinch, were he now to open his full song upon us, would be as appalling as the tones of the owl in the midst of a fine summer's day.

III.

Mr. JOHN HUNTER, my late valued friend, and honoured preceptor, under whose roof I first caught a gleam of that light which so successfully conducted him through the obscure paths of nature, first demonstrated the different sizes of the testes of birds at different seasons of the year. On a farther investigation of this subject, a fact presented itself to me, which may not be unworthy of the attention of this Society, and, as it is in some measure connected with the preceding observations, I have taken the liberty of annexing it.

In those birds that remain but a short time paired with the female, there appears a vast disproportion in the size of the testes, compared with those that live in the connubial state much longer. The cuckoo and the swift point out the fact most obviously. The common brown wren, which remains united with its female from the early part of spring, until the autumn, exhibits testes very far exceeding in size, either those of the cuckoo or the swift. The cuckoo, although a polygamist, may here be considered in the same point of view as the birds that pair. The time which he devotes to the female being so very short, more so indeed by some weeks than even that of the swift, the testes are formed extremely small in proportion to the size of the bird. I never saw them exceed in size the common vetch, while those of the wren were full as large as a common sized garden pea. The medium weight of the cuckoo is about four ounces and a half, that of the wren but little more than three drachms.*

* Ornithologists might easily have given us the weight of a bird with greater precision, by divesting the stomach of its contents, previous to the bird being

The testes of the swift, which assume a singular oblong shape, somewhat exceed the cuckoo's in bulk, though not so large as those of the wren. I have selected the wren as an example for this comparison, on account of its diminutive size. The testes of all those birds which are capable of producing young more than once in the breeding season, become tumid, as far as I have seen, in the same proportion as those of the wren.

As there are many birds, which, if unmolested, produce but one nest of young ones in the course of the season, it may be asked, why nature should cause as great an enlargement of the testes in these, as those which breed more than once; and why they should exceed in bulk those of the cuckoo or the swift? The answer, I presume, is obvious. Should any ill accident befall the nestlings of the swift when advancing to maturity, the injury would be irreparable, the parent bird being destined to quit the country before another offspring could be reared. The cuckoo is in the same predicament; but the wide dispersion of its young ones, (being placed singly in the nests of other birds), gives them such security as almost to preclude the possibility of their general destruction.* But it is not so with those birds which make a longer stay; should similar accidents befall them, they can repair their losses. Nature, as long as incubation could serve their

weighed. For example: how very different must the weight of the owl be, which, in its nocturnal flights, had the luck to pick up a mole or two, compared with that which had met with opposite fortune; or of the falcon, that had picked the bones of a leveret, or of the one that was killed with an empty stomach.

* May not this be offered as another reason, why its eggs and young ones are entrusted to the fostering care of so great a variety of birds? It could not have time, during its short stay, to rear so large a progeny; and by no other means could it have placed its numerous brood so much out of the way of danger.

purposes, would keep an accumulation of the proper powers in store, which, in the case of the cuckoo and swift, would be entirely useless.

Whether there be a regular gradation in the size of the testes (that of the bird itself being considered), throughout the whole race, in proportion to the time taken up in pairing, I cannot determine, not having had an opportunity of subjecting the matter to a full investigation. However, I thought the fact already shown of sufficient importance in natural history, to be worthy of communication, as it forms a kind of sequel to Mr. HUNTER's paper on the subject.

With due deference to the late Dr. DARWIN, I am inclined to think that the opinion he set forth respecting the pairing of cuckoos, was taken up hastily, and that the birds which his friend saw were not cuckoos feeding their nestlings, but goat-suckers, whose mode of nesting corresponds with the relation given, and whose appearance might be mistaken for them by one not perfectly conversant with the plumage and the general appearance of cuckoos when on the wing. Is it probable that the cuckoo, which is invariably a polygamist, and never pairs, nests, or incubates in this part of the island, should fall into opposite habits in another part?

To recapitulate the substance of my observations. I have first adduced some arguments in support of migration, the fact itself not being generally admitted by naturalists of celebrity, and also against the hypothesis of a state of torpor, or what may be termed the hybernating system. I have represented that the swallow tribe, and many other birds that absent themselves at stated periods, return annually

to the same spot to build their nests; and at the same time that any inference drawn from this fact in support of a state of torpor, would be fallacious upon physiological principles. That certain periodical changes of the testes and ovaria, are the inciting causes of migration. I have stated many facts, hitherto, I believe, unnoticed, chiefly with respect to the *cause* which excites the migrating bird, at certain seasons of the year, to quit one country for another, (viz.) the enlargement of the testes in the male, and ovaria in the female, and the need of a country where they can for a while be better accommodated with succours for their infant brood, than in that from which they depart. It is also attempted to be shown, that their departure from this country is not in consequence of any disagreeable change in the temperature of the air, or from a scarcity of their common food, but the result of the accomplishment of their errand, *i. e.* the incubation, and rearing of their young, and the detumescence of the testes and ovaria. That successive arrivals of migrating birds are attributable to the progressive developement of the generative system in the male and female: that progressive developements are wise provisions of nature; that premature arrivals and departures are frequently to be accounted for on the same principle; that the departure of the spring migrators is owing to a change in the testes and ovaria, the very opposite to that which took place in the spring; that the departure of the young birds is not guided by the parent, but the result of an unknown principle.

In the second part of this paper, some observations are made on the winter birds of passage; that they quit their

homes (this country) in spring, in quest of a country better suited to their intended purpose than their own; that they are actuated by the same impulse in quitting this country, that causes the spring birds to come to it, and that want of food cannot be the inducement; that the emigration of the winter birds is less complete than that of the others (the spring migrators); that some species breed here, especially the wild-duck and wood-pigeon; that the redwings and fieldfares are the most regular and uniform in their appearance and disappearance, and most probably never risk the trial of incubation here;* that they quit the country *temporarily* in severe and long continued frost through want of food, and return to it again at the approach of more temperate weather; that the arrival of water-birds forebodes the approach of intense frost, the usual return of the winter-birds, a thaw; that examinations of the latter prove them to have taken long flights before their return, and sets the fact of temporary migration beyond the reach of doubt.

I have then made a digression, and introduced some observations on the singing of birds: and in a third part, given some additional particulars respecting the different sizes of the generative organs of birds, as they appear at different seasons of the year.

* I must be understood by the word "here," to mean that part of Gloucestershire under my own observation.

III. *On the nature of the acid and saline matters usually existing in the stomachs of animals.* By William Prout, M. D. F. R. S.

Read December 11, 1823.

That a free, or at least an unsaturated acid usually exists in the stomachs of animals, and is in some manner connected with the important process of digestion, seems to have been the general opinion of physiologists till the time of Spallanzani. This illustrious philosopher concluded, from his numerous experiments, that the gastric fluids, when in a perfectly natural state, are neither acid nor alkaline. Even Spallanzani, however, admitted that the contents of the stomach are very generally acid; and this accords not only with my own observation, but with that, I believe, of almost every individual who has made any experiments on the subject.

With respect to the nature of this acid, very various opinions have been entertained. Some of the older chemists seem to have considered it as an acid, *sui generis;* by others it was supposed to be the phosphoric, the acetic, the lactic acid,* &c. No less various have been the opinions respecting

* After I had discovered the principal fact related in this paper, I was surprized to find how nearly Scopoli had come to the same conclusion. He did not indeed come to the conclusion, as far as I can ascertain, that free muriatic acid exists in the stomach, but he advanced the opinion, that the muriatic acid, in union with ammonia, found in such abundance in the stomach of ruminating animals, is secreted by that organ itself. The only account of Scopoli's experiments I have seen is in Johnson's Animal Chemistry, i. 183.

its origin and use; some supposing that it is derived from the stomach itself, and is essential to the digestive process; others, that it is derived from the food, or is a result of fermentation, &c.; in short, there seems to be no physiological subject so imperfectly understood, or concerning which there has been such a variety of opinions.

The object of the present communication is to show, that the acid in question is the *muriatic acid*, and that the salts usually met with in the stomach, are the alkaline muriates. As to the origin and use of these principles, as well as the occasional appearance of other acids, &c. in the stomach, I reserve what I have to say on these subjects till a future opportunity, and shall merely remark at present, that the facts now adduced seem to be intimately connected, not only with the physiology and pathology of the digestive process, but with other important animal functions.

Having ascertained the circumstances above mentioned in a general manner, and by means which it would be here unnecessary to detail, an attempt was made to contrive some unexceptionable method by which their truth might not only be satisfactorily demonstrated, but at the same time that the relative quantities of the different principles might be determined: after various attempts, the following processes were adopted for these purposes.

The contents of the stomach of a rabbit, fed on its natural food, were removed immediately after death, and repeatedly digested in cold distilled water till they ceased to impart any thing to that fluid. The whole of these different portions of fluid, which always exhibited strong and decided marks of acidity, were then intimately mixed together, and after being

allowed to settle, were divided into four equal portions. 1. The first of these portions was evaporated to dryness in its natural state, and the residuum burnt in a platinum vessel; the saline matter left was then dissolved in distilled water, and the quantity of muriatic acid present determined by nitrate of silver in the usual manner; the proportion of muriatic acid, in union with a *fixed* alkali, was thus determined. 2. Another portion of the original fluid was super-saturated with potash, then evaporated to dryness, and burnt, and the muriatic acid contained in the saline residuum determined as before. In this manner the *total* quantity of muriatic acid present in the fluid was ascertained. 3. A third portion was exactly neutralised with a solution of potash of known strength, and the quantity required for that purpose accurately noticed. This gave the proportion of *free* acid present; and by adding this to the quantity in union with a fixed alkali, as determined above, and subtracting the sum from the *total* quantity of muriatic acid present, the proportion of acid in union with *ammonia*, was estimated. But as a check to this result, the third neutralised portion abovementioned was evaporated to dryness, and the muriate of ammonia expelled by heat, and collected. The quantity of muriatic acid this contained was then determined as before, and was always found to represent nearly the quantity of muriate of ammonia as before estimated; thus proving the general accuracy of the whole experiments beyond a doubt. 4. The remaining fourth portion of the original fluid was reserved for miscellaneous experiments, and particularly for the purpose of ascertaining whether it contained any other acid besides the muriatic. The experiments abovementioned

seemed to preclude the possibility of the presence of any destructible acid; and the only known fixed acids likely to be present were the sulphuric and phosphoric; the muriate of barytes, however, neither alone, nor with the addition of ammonia, produced any immediate precipitate,* showing the absence of these two acids in any sensible quantity, and still farther confirming the results as before obtained.

In this manner the three following results, selected from a variety of others of a similar nature, were obtained.

	No. 1	No. 2	No. 3
	grs.	grs.	grs.
Muriatic acid in union with a *fixed alkali*†	·12	·95	1.71
——— *with ammonia*	1·56	·76	·40
——— in a *free or unsaturated state*	1·59	2·22	2·72
Total	3·27	3·93	4·83

These results then seem to demonstrate, that free, or at least unsaturated muriatic acid in no small quantity exists in the stomach of these animals during the digestive process; and I have ascertained, in a general manner, that the same is the case in the stomach of the hare, the horse, the calf, and the dog. I have also uniformly found free muriatic acid in great abundance in the acid fluid ejected from the human

* It may be proper to remark, that ammonia, after some time, caused a flocculent precipitate, consisting of the earthy phosphates in union with vegetable and animal matter, and that after combustion, traces of sulphuric acid, the result of that process, were very perceptible. But it is evident, from the experiment related in the text, that neither of these acids previously existed in the original fluid in a free state.

† For the sake of analogy, the chlorine, in union with the basis of the *fixed* alkali, is reduced in this table and the following to the state of muriatic acid.

stomach in severe cases of dyspepsia, as the following examples show. The original quantities of the fluids operated on, of course were various, but for the sake of comparison they are reduced, in the following table, to one pint, or 16 fluid ounces, which quantity, in three instances, (selected from many others) was found to contain of

	No. 1.	No. 2.	No. 3.
	grs.	grs.	grs.
Muriatic acid in union with a *fixed alkali*	12·11	12·40	11·25
——— *with ammonia**	0· 0	0· 0	5·39
——— in a *free or unsaturated state*	5·13	4·63	4·28
Total	17·24	17·03	20·92

* I have never in more than one instance, (No. 3, of the above table) been able to detect any sensible quantity of the muriate of ammonia in the fluids ejected from the human stomach; and upon enquiry of Sir ASTLEY COOPER, who was kind enough to furnish me with the fluid for examination, I was informed that the patient was in the habit of frequently taking ammonia as a medicine.

IV. *On the north polar distances of the principal fixed stars. By* JOHN BRINKLEY, *D. D. F. R. S. &c. Andrew's Professor of Astronomy in the University of Dublin.*

Read December 18, 1823.

THE apparent disagreement of the Catalogues of North Polar distances of the fixed stars, as given by different astronomers, has lately excited considerable attention. Many persons may be induced to imagine, that the means of making observations are not in so perfect a state as has been supposed.

The following examination of some important points relative to this subject, will, I hope, be deemed not unworthy of the notice of the Royal Society.

A comparison of the North Polar distances of Mr. POND and Mr. BESSEL, with my own, may give occasion to some useful enquiries. It will give me an opportunity of stating the results of my researches relative to southern motion, to which my catalogues of 1813 and 1823 are, as is known, quite opposed.

In discussing these subjects, I hope I shall be considered as searching after truth, not as handling a useless controversy, than which nothing can be more injurious to science. It will be necessary for me to enter into a considerable detail, I shall therefore briefly state the objects of the following enquiries.

Of the recent Catalogues that have been formed of the principal fixed stars, two, those of Dublin and Greenwich, agree very exactly. That of Mr. BESSEL differs considerably: but the differences are such that they would agree by a modification of the constants of refraction used. This leads me to some considerations respecting the different modes in which my Tables of Refraction, and those of Mr. BESSEL, have been constructed. I do not venture to decide which Catalogue will ultimately be found more correct, that of Dublin, and consequently that of Greenwich, or that of Konigsberg.

Mr POND, however, does not admit the agreement of the Dublin and Greenwich Catalogues, because we use different refractions, and for comparison, takes my column of North Polar distances, computed by BRADLEY's refractions.* From the differences then resulting, he infers a flexure of my instrument But that such reasoning is inconclusive, will, I think, appear from what I shall afterwards state.

In asserting the general agreement of the Catalogues of Dublin and Greenwich, both for 1813 and 1823, I mean, they agree within certain narrow limits. The mean of the differences of the Catalogues of 1813 is only a few tenths of a second. The mean of the differences of the Catalogues of 1823 is still less. It must therefore at first view appear extraordinary, that from the comparison of the two Catalogues

* It ought to be noticed that Mr. POND, in his paper in the First Part of the Philosophical Transactions for 1823, has omitted to state distinctly, that the polar distances he reasons on respecting the flexure of the instrument, &c. are not what I consider as my polar distances. In one Table, indeed, he puts "by BRADLEY's refractions" at the head. But even here a reader might suppose that they were the North Polar distances as given by me. In the same Table he places BESSEL's unchanged, by the side of mine changed, and compares them together.

of Greenwich a southern motion is deduced, whereas none appears from a comparison of the two Dublin Catalogues; but this is easily explained by an examination of the Catalogues.

From the weight of external testimony that I shall adduce, I think it will readily be conceded to me, that the southern motion does not exist. It will follow, therefore, that the mean Southern motion must be regarded as an error belonging to one or both of the Greenwich Catalogues of 1813 and 1823. It may be inferred, that the mean error principally belongs to the Catalogue of 1813, as the mean exactness of the Greenwich Catalogue of 1823 may be inferred from its agreement with the Dublin Catalogue of 1823. This is the only way it can be inferred. The observations by reflection only go to prove a relative exactness; for, in consequence of the Pole Star not having been observed at Greenwich by reflection, it was necessary for Mr. POND to assume the latitude of Greenwich, more or less, to accommodate it to the mean error of the Catalogue.

In my researches relative to the Southern motion, I have been able to avail myself of the result of important observations by Dr. BRADLEY, made at Wanstead, in 1728; of zenith observations made in France, in 1740; of Dr. MASKELYNE'S observations at Schehallien, in 1774; of General MUDGE'S observations with the zenith sector, in 1802; and of General LAMBTON'S zenith distances observed in the Mysore, in 1805.

All these observations were made with instruments not inferior to the zenith sector with which BRADLEY so exactly ascertained the quantity of the aberration of light, and it is not necessary for my purpose to suppose them superior.

It has been said, that the Westbury observations of Mr. Pond confirm the Southern motion, as also a few stars observed by Mechain, in the late French measurement. But the irregularities to be found in comparing the Westbury Catalogue with the two Catalogues by the Greenwich mural circle, show that the former cannot be of any use in this enquiry. The few French results that appear to support the Southern motion, are opposed to other results by better instruments.

The Palermo Catalogue, published by M. Piazzi, as containing the correct result of all his observations, when compared with Bradley's Catalogue of 1755, and the two Dublin Catalogues afford a remarkable testimony in favour of the uniformity of the annual variation in declination of the principal stars.

This result of the question of the Southern motion, appears adverse to the opinion advanced by Mr. Pond, relative to the decided superiority of the Greenwich over the Dublin circle. If we are to judge of the instruments by the observations, I am probably right in the opinion I have long entertained, of the unfitness of the Greenwich circle for the accurate investigation of small motions. Whereas I have generally found my instrument consistent in that respect; unless it be said, it has deceived me in regard to the parallax of α Lyræ. This, resting on the authority of the Greenwich instrument, I am not at present disposed to admit. I had intended concluding with some notices respecting Mr. Pond's paper "on the Parallax of α Lyræ," read before the Royal Society on the same day as that relative to the Southern motion; but as that paper requires to be particularly remarked on, I

shall here confine myself to the consideration of the North Polar distances, and as connected therewith of the Southern motion.

On the Catalogues of North Polar Distances.

I have placed, in Table I., beside each other, the North Polar distances observed at Greenwich about 1813, and at Dublin about the same time, together with the differences. In like manner have been placed also the Greenwich and Dublin North Polar distances of 1823, together with the differences. An inspection of these will show, except in one or two instances, a very extraordinary agreement. Many of the Polar distances differ by less than 1″; and with the exception of Sirius, in the Catalogue of 1813, the differences are never greater than what might arise from accidental circumstances. The Greenwich Catalogue of 1813 is *less* in its mean quantity than that of Dublin by 0″,47, and the Greenwich Catalogue of 1823 is greater than that of Dublin in its mean quantity by 0″,10. But we are to consider that these Catalogues are computed by different tables of refraction. The constant of refraction $\left(\frac{m-1}{\text{Sin. }1''}\right)$ in BRADLEY's table (that used by Mr. POND) is 57″. In my Table it is 57″,72

My constant has been determined by the circle and the meteorological instruments used here, and therefore must necessarily be adopted for my observations. When an astronomer has found the constant of refraction by his own instruments, his Catalogue of North Polar distances ought to be formed independently of any other instrument or table of refractions. No partial change can be admitted. Mr. POND,

however, as has been mentioned, has done otherwise, and applied BRADLEY'S refractions to my Catalogue.*

I particularly regret this circumstance, because it has occasioned my Catalogue to appear to differ more from that of Mr. BESSEL, than it really does. The differences that actually exist are sufficiently difficult to account for. Indeed had Mr. POND also reduced the Catalogue of Mr. BESSEL by the same refraction, the differences would have appeared much better. But this mode of proceeding would not have been less objectionable. From the differences between his own Catalogue and my Catalogue reduced, Mr. POND infers that my telescope is subject to flexure by the quantity of the difference at each zenith distance. Now it must appear a very extraordinary law, and not easily reconcilable to any mechanical principle, that the flexure should be nearly as the tangent of the zenith distance. This it must necessarily be according to his method of changing my North Polar distances.

It is evident, by comparing the two Catalogues, that there is no difference between them but what might arise from unavoidable errors. Had each star been exact to the tenth of a second, still Mr. POND'S reasoning would have led him to do the same. He would have reduced them by BRADLEY'S

* It may be said, that in a Paper in the Transactions of the Royal Irish Academy, about eight or nine years ago, I changed *my own* North Polar distances for the purpose of comparison. But the circumstances are entirely dissimilar. I have always referred to, and always used, the North Polar distances computed by my own refractions.

Mr. BESSEL, in his comparison of my polar distances with his own, does not change mine to adopt his own refractions. He knew I had determined my own with my own instruments.

refractions, and so made the Catalogues differ. He attributes the differences to flexure. Now he admits that the flexure would be the same at equal distances on each side of the zenith; but it does not appear to have occurred to him that my refractions were determined by observations of circumpolar stars to the north of the zenith by the same instrument, and that therefore they must be *exactly* in error by the quantity of flexure; and so when applied to stars south of the zenith, must *exactly* compensate for the effects of flexure.* Mr. Pond did not perceive that what he took away with one hand, he ought to have restored with the other, and so left my Catalogues as he found them.

It is difficult to say how far the difference of our constants of refraction may be occasioned by a discordance in the meteorological instruments. This should be enquired into. It is still more difficult to imagine a difference in the mean refractions at the two places.

* The manner in which the telescope and circle are attached in the Dublin instrument, appears to preclude all probability of flexure in the telescope. Indeed it does not appear a matter of much difficulty where the telescope and circle are combined together, as in the Dublin and Greenwich instruments, to guard against a flexure in the telescope. If talents such as those of Mr. Ramsden and Mr. Troughton have been unable to provide against the flexure of the telescope, it appears to me quite useless to expect exactness in the other parts of the instruments. Therefore, it might be considered as almost a waste of time to endeavour to overcome the difficulties I should have to encounter here by observing by reflection. The difficulties in general would be greater than at Greenwich; and, above all, among the few clear days that occur, very few could be found sufficiently calm to observe by reflection.

Mr. Pond had a motive for pursuing this mode of observation which does not exist here; he had no other method of determining his zenith point. I do not consider the observations by reflection necessary for my own satisfaction, but if they be for that of others, I should not object to undertake them.

In whatever way the subject is considered, the coincidence of the Greenwich and Dublin Catalogues speaks in the strongest manner for the excellence of the divisions of both instruments.

This coincidence will, if I mistake not, appear in a stronger point of view, by deducing the co-latitude of Greenwich from applying the zenith distance of each star, as observed by reflection at Greenwich, to the Polar distance of the same star as given in the Dublin Catalogue for 1823. The results are given in Table 2. The mean of the 30 stars is $38^\circ\ 31'\ 20'',8$, or two tenths of a second less than that assumed by Mr. Pond, and four tenths greater than that found by Mr. Bessel, *from Dr.* Bradley's *observations.*

The difficulty that has arisen from the comparison of the Greenwich and Dublin Catalogues with that of Mr. Bessel, is now to be considered. In this also, there will, I think, be nothing found adverse to that degree of accuracy, which is supposed to belong to modern instruments and modern observations.

It will readily appear, that the differences between the Dublin Catalogue and that of Mr. Bessel, are equivalent to a change in the constant of refraction of about one second. If, in computing the Dublin observations, we increase my constant of refraction by half a second, and in computing the Konigsberg observations, we decrease Mr. Bessel's constant of refraction by half a second, the Catalogues will be found to agree sufficiently.

It is not necessary to search for other causes till we are assured this is not the true one. The investigation of the

exact constant of refraction will be found one of great difficulty, if we consider the nature of it. Mr. BESSEL and I have proceeded by different methods, and, in some respects, my method appears more likely to lead to an accurate result.

Mr. BESSEL's object is to obtain a formula that shall embrace all elevations from the zenith to the horizon, and, therefore, he necessarily assumes a law of variation of density in the atmosphere.

In my investigation, I only consider zenith distances not greater than about 75° or 76°, where no sensible effect is produced from our ignorance of the law of variation of density. Let us consider the advantages and disadvantages of each method.

Mr. BESSEL* supposes the equation of density to be

$$\rho = (\rho)\, e^{-\frac{g-l}{g-l}\, as}$$

ρ being the density at the height, *as* & (ρ) that at the surface, a being the radius of the earth, and l the height of an uniform atmosphere. He proposes to find g, so that the formula of refraction deduced may satisfy the observations. He has therefore two unknown quantities, g, and the constant of refraction, k.

When we consider the irregularities of refraction at low altitudes, and the number of observations required to make those irregularities disappear, it may be thought that the problem is unnecessarily involved by requiring the investigation of two unknown quantities, and, under the circumstances of the case, there is reason to suppose that the observations may be satisfied within certain small limits, by assigning values

* Astronom. Fundament. p. 28.

to k, even differing 1″, by making corresponding changes in g, so that the problem partakes too much of the nature of an indeterminate one. Thus the advantage apparently gained by large refractions, is lost by attendant inconveniencies.

In my investigation, there is only one unknown quantity, but then I have much smaller quantities to work with.

Theory gives as far as about 76°, whatever be the law of variation of density in the atmosphere.

* The mean refraction $(r) = k \tan. z - \frac{k\,l \tan. z}{a \cos.^2 z}$ &c. (1), z being the zenith distance.

By a table of refractions, or by the pole star, and a star or stars more remote, k is easily obtained nearly. Then if the true value be $k + dk$

$$dr = dk \tan. z \ (2) \text{ sufficiently exact.}$$

Let A and B be the observed zenith distances of a circumpolar star, (considering B negative when south of the zenith) above and below the pole, R & R′ the refractions *exactly* computed by the formula (1), k being the approximate constant of refraction.

Then by (2)

$$\text{Co-lat.} = \frac{A + B + R + R' + dk \tan. A + dk \tan. B}{2}$$

Hence, if C represent the mean co-latitude thus determined by circumpolar stars remote from the pole, and N that by stars near the pole, we obtain an equation of the form

$$C + m\,dk = N + n\,dk$$

$$\text{and } dk = \frac{N - C}{m - n}$$

In this investigation the Z. D. of the stars remote from the Pole, should not be greater, when below the Pole, than about

* Transactions of the Royal Irish Academy, Vol. 12.

76° or 77°, and not less than about 69° or 70°. Let us suppose it in its mean quantity at about 73°, and then the value of m will be about $\frac{\tan. 73°}{2} = 1{,}6$, and for the stars near the Pole, the mean value of n about 0,7. Hence $m - n$ is less than unity, and, consequently, the error of the constant of refraction is greater than the error of N — C. Now I think it will be conceded to me, that it requires the exactest instruments and exactest observations to determine the quantity N — C certain to half a second. A greater number of stars can be used for determining C than for N, but then the greater zenith distances will probably occasion C to be more inexact than N. In C, the irregularities of refraction, and in N, the errors of division, have most influence.

In a series of observations in which I am at present engaged, for determining anew the constant of refraction, I use for N the Pole star only, and I lessen the effect of the errors of division, that may be apprehended, by being enabled to observe the Pole star in all parts of its daily course.

I shall not anticipate here the probable result of these observations.

The object of the above, is not to examine whether the constant of refraction has been determined with greater exactness at Dublin or at Konigsberg, but only to endeavour to show that the uncertainty, which exists, cannot be considered in any manner adverse to the received opinion respecting the exactness of modern observations and modern instruments.*

Before leaving this subject, I may be permitted to make a

* A catalogue of Polar distances necessarily exhibits, for low stars, the error of the constant of refraction as it were considerably magnified.

few remarks relative to one circumstance, that Mr. Bessel relies on a good deal, as proving the exactness of his refractions, viz. that they give the obliquity of the ecliptic at the Winter and Summer solstices the same. Whereas other Tables of refraction give the obliquity in Winter less than in Summer.

We have lately commenced here to observe the zenith distance of the sun every day at noon, on which it can be seen.

I had formerly been unwilling to observe the sun with the circle, except at the solstices, as I considered the heat likely to derange the instrument for my observations relative to parallax.

The Dublin circle, in one respect, is well adapted for observing the sun. By observing a few minutes before and after noon, four observations give me the zenith distance of the centre, independently of the semi-diameter, or correction for collimation.

Observations on eighty-seven days have been obtained during the last year. The manner in which I have used them, is, I believe, somewhat new. With the declination in the Nautical Almanac, and the meridional zenith distance deduced from the observation, I obtain the latitude of the Observatory. I assume, that the declination in the Nautical Almanac is only erroneous by an error in the longitude (L) of the sun, and obliquity of the ecliptic (O). Then, for each day, I have the lat. = Z. D. observed $\pm$ decli. $+ m\,d\,L + n\,d\,O + p\,dk$. From the nature of the Solar Tables it may be assumed, and the assumption is exact enough for my en-

quiry, that dL only arises from an error in the place of the equinox.

The mean of the latitudes thus found during the year, will be affected by an error $s\,dL + t\,dO + v\,dk$, in which the coefficients s and t are so small, that the effects of dL and dO will be insensible. Thus the eighty-seven observations give my latitude $= 53^\circ\ 23'\ 12'',39 + 0,04\,dL + 0,21\,dO + 1,42\,dk$.

By circumpolar stars remote from the Pole

Co-lat. $= 36^\circ\ 36'\ 47'',15 + 1,62\,dk$,

making the sum $= 90^\circ$,

we deduce $dk = 0'',15 - 0,01\,dL - 0,07\,dO$.

This small value of dk appears to confirm the accuracy of the constant k that I had used. But if I relied on this I should deceive myself; for on examining the series of latitudes deduced, it is evident that this coincidence arises from the circumstance of more observations having been made while the sun was on the north side of the equator, than while on the south. The latitudes deduced show clearly, that had more observations been made nearer the winter solstice than the summer, the value of dk would have been much more considerable.

This contradictory result, and some other circumstances that appear on an examination of the latitudes deduced, seem to point out that some new equation is required to be applied for the solar refraction. At least, that no conclusion can be drawn as to the exactness of a table of refractions, from its giving the obliquity of the ecliptic the same at the two solstices.

On the Southern Motion.

In Table III. are given the North Polar distances of forty-six principal stars, from recent observations with the circle of the Observatory of T. C. Dublin, and also the North Polar distances of the same stars from the Catalogue of 1813* reduced to 1823.†

The column of differences shows that there are none greater than what may be attributed to accidental circumstances, especially when it is considered that the Catalogue of 1813 was formed from a small number of observations of each star. The mean difference $= + 0'',2$, whereas the mean difference of Mr. Pond's Catalogues $= + 1'',1$. In this then our instruments are at variance. The discordance appears much more striking if we examine the differences that exist as to certain stars. It is from these, unless I am much mistaken, I shall be enabled to show the greater exactness of the Dublin instrument. But it may be useful to add a few remarks respecting the *mean difference*, to show there are reasons for supposing a constant error, which, being allowed for, would considerably reduce the above mean difference of $1'',1$.

By Table I. it appears, the mean difference between the Dublin Catalogue of 1813, and the Greenwich Standard Catalogue of $1813 = + 0'',47$. The mean difference between

* Trans. R. I. Academy, Vol. 12, p. 69.

† To the Catalogue which was published in the Journal of Science, October, 1822, have been since added several stars, viz. α Persei, Rigel, α Hydræ, 2 α Libræ, α Herculis, and α Pegasi. In that Catalogue, the mean difference from that of 1813 was exactly $0'',0$.

the Catalogues of 1823 = − 0″,10. Now, supposing for a moment these differences are errors belonging to the Greenwich instrument; that is, the Catalogue of 1813 is in defect = 0″,47, and the Catalogue of 1823 is 0″,10 in excess. Here then the mean southern motion would be reduced by 0″,6, and there would remain only 0″,5 to be accounted for, half of which *might* be accounted for by a circumstance to be mentioned presently.

The error I suppose in the Catalogue of 1823 is so small, that the observations by reflection cannot be adduced to controvert it; this, as I have mentioned, could not even be done had the supposed error been much greater, in consequence of the latitude having been assumed. The observations by reflection have only shown the consistency of the North Polar distances, not their absolute exactness.

The N. P. D. of the Pole Star in the Standard Catalogue of 1813

	°	′	″
	= 1	41	21,6
10 years variation = 19″,457 × 10		− 3	14,6
Predicted, 1823	1	38	7,0
Observed, 1823	1	38	7,5
			0,5 South.*

Mr. POND has not remarked this apparent southern motion of the Pole star, which is so nearly equal to the sum of the mean differences of the Greenwich and Dublin Catalogues of 1813 and 1823. It is highly probable, that this apparent southern motion of the Pole star has arisen from small errors in determining the place of the Pole star at each period.

* Excepting error, if any, from lunar nutation.

It will be remarked in the Catalogue, Table III., that the Pole star, as determined with the Dublin instrument at the two epochs, agrees exactly.

Predicted, 1823,	1° 38′ 7″,3	
Observed,	1 38 7 ,3	

This is a mean between Mr. Pond's observed and predicted places.

A circumstance above alluded to is of some importance. In Table IV. will be found the annual variations as found by Mr. Pond, by myself, and by Mr. Bessel. Mine are between those of the other two, but nearer to Mr. Pond's than to Mr. Bessel's. The effect of this would be, as to mine, to reduce the mean southern motion of Mr. Pond, about a quarter of a second; but if Mr. Bessel's annual variations be adopted, they would, in conjunction with the above supposition relative to the Pole star, intirely take away the mean southern motion of Mr. Pond's Catalogue.

It will be found, I conceive, difficult, in forming a Catalogue of stars by a mural circle, to avoid a small constant error, and if the Greenwich observations of the Pole star be consulted from the beginning, we shall find enough to induce us to suppose, that such errors may exist in one or both of the Greenwich Catalogues of 1813 and 1823.

In respect to the annual variations, I shall not venture to give an opinion whether Mr. Pond's or Mr. Bessel's be more exact. I shall only state that mine, which are generally between the two, were formed, as will easily be seen on examination, by a careful comparison of my Catalogue of 1823, with the Catalogue* deduced by Mr. Bessel from Dr. Bradley's observations.

* Astron. Fundament. p. 138, &c.

The above remarks, relative to the mean difference of Catalogues, have been adduced only because I hope they will be found to contain some useful illustration on this subject.

The proofs I shall now bring of the non-existence of a southern motion, are derived from comparing, in years remote from each other, the places of particular stars, supposed by Mr. POND to have a considerable southern motion, with others supposed to have none, or only a very small southern motion. Whatever doubt may arise when we reason on such small quantities as the mean difference, none can occur with respect to several particular stars that have been supposed to have a great southern motion.

The conclusion that follows is, that there is no southern motion similar to what Mr. POND has deduced. There may be certain stars of which the proper motions are not uniform. In some stars these may have a tendency to diminish, and in others to increase, but nothing of this kind is as yet certainly known. Perhaps, hereafter, it may be confirmed that the proper motion of Procyon is increasing.

(I) The stars α Cassiopeæ and γ Ursæ Majoris, are particularly considered by Mr. POND. According to him, α Cassiopeæ appears to have a considerable southern motion relatively to γ Ursæ Majoris.

It is a somewhat singular circumstance, that Dr. BRADLEY observed, with great care, at Wanstead, in 1727 and 1728, the difference of declination between these two stars. It is worth while to quote his own words.*

" But as it may be of some use to future astronomers to
" know what were the mean differences of declination, at a
" given time, between some stars that lie nearly opposite to

* Phil. Trans. Vol. 45. Old Abridg. Vol. 10, p. 51.

" one another in right ascension, and not far from either of " the colures, I shall set down the result of the comparison " of a few that differ so little in declination, that I could " determine the quantity of that difference with great cer- " tainty." He then states, that the *mean* difference of declination was 10′ 28″,1, on March 27, (old stile) 1727. This, reduced to January 1, 1727, new stile, is 10′ 38″,4.

The declinations of these stars in 1755, reduced from Dr. BRADLEY's observations with the Greenwich quadrant, by Mr. BESSEL,* are

α Cassiopeæ	55° 11′ 23″,7
γ Ursæ Maj.	55 3 24 ,4
Difference	7 59 ,3

Dr. MASKELYNE observed these stars at Schehallien, 1774. The observations† of the zenith sector, reduced to January 1, 1774, by the usual equations, give

Z. D. α Cassiop.		Z. D. γ Ursæ Maj.	
Oct. 2	1° 22′ 45″,5	Oct. 14	1° 43′ 22″,2
3	43 ,5	15	25 ,4
5	45 ,7	18	23 ,4
24	46 ,4	22	22 ,3
Mean	1 22 45 ,3	Mean	1 43 23 ,3
			1 22 45 ,3
		Diff. decl.	20 38 ,0
		Diff. ref	+ 0 ,4
			20 38 ,4

M. PIAZZI,‡ Palermo.
Declination, 1800.

γ Ursæ .	54° 48′ 23″,0
α Cassiop.	55 26 17 ,6
Diff.	37 54 ,6

* Astron. Fund. pp. 140, 208. † Phil. Trans. Vol. 65.

‡ M. PIAZZI's Great Catalogue, " Panormi, 1814."

Hence this Table

		Observed Difference of Declination.	Variation in 10 Years.	At	Reduced to 1780.
		′ ″	′ ″		′ ″
1727	Dr. BRADLEY, Wanstead.	+10 38,4	6 39,2	1741	6 39,0
1755	Dr. BRADLEY, Greenwich.	7 59,3	6 39,5	1764	6 39,4
1774	Dr. MASKELYNE, Schehallien.	—20 38,4	6 38,6	1787	6 38,6
1800	M. PIAZZI, Palermo.	—37 54,6	6 38,4	1812	6 38,5
1823	Dr. BRINKLEY, Dublin.	53 11,0			

The last column is deduced from the fourth, by computing from* the secular variation of annual precession in diff. decl. Table III. $= + 0'',067 - 0'',029 = + 0'',038$.

The mean of the last column is $6' \, 38'',9$, the same as that deduced by comparing the Greenwich observations of 1755, with the Dublin of 1813.

The variations in the last column agree so nearly, that there cannot be a doubt that the apparent motions of declination of these stars have been uniform for upwards of ninety years.

(II.) The observations made in France with a sector, in 1739 and 1740,† appear to have been exact, by comparing the amplitudes of the same arc determined by different stars.

The lunar nutation was then unknown: but if we correct the observations for this, and solar nutation, we may then deduce the differences of declination of certain stars, and compare them with later observations.

According to Mr. POND, Capella has a considerable south-

* The secular variation is here and elsewhere given retrospective.

† La Merid Verif. p. lxxxiii., &c.

ern motion relatively to η Ursæ Majoris, viz. at the rate of 1″,9 in ten years, at 1818 greater than at 1784.

(I.) Capella and η Ursæ Majoris observed at Paris.

Z. D. Jan. 1, 1740.

Capella	3° 8′ 28″,6 S.	η Ursæ Maj.	1° 47′ 7″,7. N.
Sol. and lunar nut.	— 5 ,7		7 ,6
	3 8 22 ,9		1 47 0 ,1 N.
			3 8 22 ,9 S.
		Diff. decl.	4 55 23 ,0*

		Greenwich, 1755, decl.		Palermo, 1800 decl.		Dublin, 1823, N.P.D.
Capella .		45° 43′ 4″,8		45° 46′ 37″,5		44° 11′ 36″,2
η Ursæ Maj.		50 32 39 ,0		50 18 59 ,2		39 47 59 ,9
Diff.		4 49 34 ,2		4 32 21 ,7		4 23 36 ,3
Paris, 1740		4 55 23 ,0		4 49 34 ,2		4 32 21 ,7
	(15 years)	5 48 ,8	(45 y.)	17 12 ,5	(23 y.)	8 45 ,4

The secular variation of annual precession in diff. of declination for these two stars is $+,622 + ,153 = +,675$. Vide Table III.

Hence,

Rate at 1747	in ten years	3′ 52″,5	reduced to 1780	3′ 50″,3
at 1777		3 49 ,4		3 49 ,2
at 1812		3 48 ,4		3 50 ,5

* The result from the observations at Bourges is 4° 55′ 22″,8. This close agreement must be accidental. But the observations in general may be considered as exact. It is worth while mentioning here the latitude of the Royal Observatory, at Paris, as deduced by comparing the Dublin north polar distances 1823, with the zenith distances of three stars observed at Paris, 1739 and 1740,

Capella gives lat. =	48° 50′ 14″,0
η Ursæ Maj. - - - -	13 ,1
γ Draconis - - - - - -	17 ,4
Mean	48 50 14 ,8

This differs so little from 48° 50′ 14″, the lat. according to the latest determination, that it shows also we can calculate the motions of these stars pretty exactly for eighty years.

This is as great a coincidence, allowing for unavoidable errors, as could be expected from the most uniform variation. The difference that exists between the 2nd and 3rd, is contrary to a southern motion

(2) γ Draconis and α Cygni,

	Dunkirk, 1740, Z. D.		Greenwich, 1755, decl.		Palermo, 1800, decl.		Dublin, 1823, N.P.D.
γ Draconis	0° 29′ 47″,0 N		51° 31′ 40″,6		51° 31′ 4″,5		38° 29′ 10″,3
α Cygni -	6 40 12 ,8 S		44 24 56 ,7		44 34 19 ,8		45 20 52 ,0
Diff.	7 9 59 ,8		7 6 43 ,9		6 56 44 ,7		6 51 41 ,7
			7 9 59 ,8		7 6 43 ,9		6 56 44 ,7
		(15 y.)	3 15 ,9	(45 y.)	9 59 ,2	(23 y.)	5 3 ,0

The secular variation of annual precession in diff. of decl. $= +,202 -,227 = -,025$.

Hence,

Rate at 1747	in ten years	2′ 10″,6	reduced to 1780	2′ 10″,7
at 1777		2 13 ,1		2 13 ,1
at 1812		2 11 ,7		2 11 ,6

The coincidence here is not so great as before, but there is nothing the least in favour of a southern motion in α Cygni.

(III.) The observations* made by the late General MUDGE, with the zenith sector, in 1802, appear to concur in evidence against the southern motion, by a comparison of the place of Capella with those of γ and η Ursæ Maj. and γ Draconis. The instrument with which these observations were made, and the care† used in making them, entitle them to great weight.

* Philosophical Transactions, 1803.

† It is necessary to remark, that the computations, as given in the Philosophical Transactions, were not made with the same care as the observations. This explains the different results which appear here. For Capella, the lunar nutation appears to have been applied the wrong way.

(1) Mean zenith distance, January 1, 1802.

	Dunnose.	Arbury Hill.	Greenwich.
γ Ursæ Maj.	4° 10′ 37″,8 N	2° 34′ 16″,8 N	3° 19′ 6″,0 N
Capella . .	4 50 21 ,7 S	6 26 41 ,4 S	5 41 51 ,0 S
	9 0 59 ,5	9 0 58 ,2	9 0 57 ,0
	Mean of the three	= 9 0 58 ,2	

	Greenwich, 1755, decl.	Mean from Z. Sect. 1802.	Dublin, Jan. 1, 1823.
Capella . .	45° 43′ 4″,8	Diff. 9° 0′ 58″,2	44° 11′ 36″,2
γ Ursæ Maj.	55 3 24 ,4	9 20 19 ,6	35 19 15 ,1
	9 20 19 ,6	(47 y.) 19 21 ,4	8 52 21 ,1
			9 0 58 ,2
		(21 y.)	8 37 ,1

The secular variation of annual precession = + 0″,622 — 0,029 = + ,593.

Hence,

Rate at 1778 } in ten years { 4′ 7″,1 } reduced to 1780 { 4′ 7″,0
at 1813 } in ten years { 4 6 ,2 } reduced to 1780 { 4 8 ,1

This discordance, contrary to the southern motion, may be safely attributed to the errors of observation.

(2) If we compare η Ursæ Majoris and Capella,

	Dunnose.	Clifton.	Arbury Hill.	Greenwich.
η Ursæ Maj.	0° 18′ 45″,5 S	3° 9′ 9″,2 S	1° 55′ 6″,2 S	1° 10′ 17″,8 S
Capella	4 50 21 ,7 S	7 40 42 ,7 S	6 26 41 ,4 S	5 41 51 ,0 S
	4 31 36 ,2	4 31 33 ,5	4 31 35 ,2	4 31 33 ,2
The Mean is	4 31 34 ,5			

We may now refer back to (II.) (1), and instead of the Palermo diff. of declination, insert this, and we shall obtain,

Rate at 1747 for ten years, gives rate for ten years at	1780	3′ 49″,9
1778 ————————————	1780	3 49 ,5
1813 ————————————	1780	3 49 ,9

This, therefore, shows a uniform variation.

(8) Capella and γ Draconis.

	Dunnose.	Clifton.	Arbury Hill.	Greenwich.
γ Draconis	0° 53′ 56″,7 N	1° 56′ 26″,9 S	0° 42′ 22″,9 S	0° 2′ 24″,6 N
Capella .	4 50 21 ,7 S	7 40 42 ,7 S	6 26 41 ,4 S	5 41 51 ,0 S
	5 44 18 ,4	5 44 15 ,8	5 44 18 ,5	5 44 15 ,6
	Mean difference	5 44 17 ,1		

	Greenwich, 1755, decl.	From Z. Sector. Mean diff.		Dublin. 1823, N.P.D.
Capella - -	45° 43′ 4″,8	- - -		44° 11′ 36″,2
γ Draconis - -	51 31 40 ,6	- - - -		38 29 10 ,3
Diff	5 48 35 ,8	5 44 17 ,1		5 42 25 ,9
		5 48 35 ,0		5 44 17 ,1
	(47 y.)	4 18 ,7	(21 y.)	1 51 ,2

The secular variation of annual precession in diff. of decl. $= +,622 + ,202 = + ,824$.

Hence,

Rate at 1778 } in ten years { 55″,0 } reduced to 1780 { 54″,9
at 1813 } { 52 ,0 } { 54 ,7

a result supporting a uniform variation.

It may not be without its use in this enquiry, to show what the latitude of Greenwich comes out independently on the mural circle, viz. by reducing the zenith sector observations of 1802 to 1823 (applying an uniform variation, viz. that deduced from the variations in 1789, by allowing the changes in precession) and then computing from the north polar distances for 1823, determined by the Dublin observations.

Mean Z. D Greenwich, Jan. 1, 1802.

	γ Draconis.	γ Ursæ Maj.	α Ursæ Maj.	Capella.
	0° 2′ 24″,6 N	3° 19′ 6″,0 N	1° 10′ 17″,8 S	5° 41′ 51″,0 S
Reduction to 1823	- 14 ,7	− 7 0 ,0	+ 6 21″,8	- 1 35 ,3
	0 2 9 ,9	3 12 6 ,0	1 16 39 ,6	5 40 15 ,7
Dublin, N. P. D.	38 29 10 ,3	35 19 15 ,1	39 47 59 ,9	44 11 36 ,2
Co-lat. Greenw.	38 31 20 ,2	38 31 21 ,1	38 31 20 ,3	38 31 20 ,5

The mean of these gives the latitude of Greenwich 51° 28′ 39″,5, or one-tenth of a second less than what Mr. Bessel found from his admirable investigations on Dr. Bradley's observations. He strongly contends for the exactness of 51° 28′ 39″,6, which is 0″,6 more than that recently assumed by Mr. Pond.

(IV.) Two stars in which Mr. Pond finds a great southern motion are γ and α Pegasi.

These two stars were observed by General Lambton, at his station of Dodagoontah, in the Mysore, at the same time that α Serpentis was also observed.* For α Serpentis, Mr. Pond finds none, or a very small, southern motion.

We have hence an opportunity of comparing the relative changes of N. Polar distance of these stars and α Serpentis.

An examination of the observations in the Asiatic Transactions will show, that for stars so near the zenith, much reliance may be had on the results of the observations.†

(1) γ Pegasi and α Serpentis.

	Greenwich. 1755, decl.		Dodagoontah. 1805, Z. D.		Dublin, 1823, N.P.D.
α Serpentis	7° 12′ 48″,6		5° 56′ 59″,6 S		83° 0′ 36″,6
γ Pegasi -	13 49 14 ,1		1 6 4 ,2 N		75 48 0 ,4
Diff.	6 36 25 ,5		7 3 3 ,8		7 12 36 ,2
			6 36 25 ,5		7 3 3 ,8
		(50 y.)	26 38 ,3	(18 y.)	9 32 ,4

* Asiatic Transactions, Vol. 10. p. 359.

† It may be said, that the observations made at Dodagoontah were not exact, judging by the irregularities exhibited by the latitude found by different stars. But these were owing to the errors of the catalogue of declination which General Lambton possessed. If the zenith distances be reduced, and applied to either of the Dublin Catalogues of N. Polar Distances, the latitudes by each star will be found to agree.

Secular variation of annual precession in diff. decl. = + ,349 + ,013 = + ,362.

Hence,

Rate at 1780 } in ten years { 5′ 19″,6 } reduced to 1780 { 5′ 19″,6
at 1814 } { 5 18 ,0 } { 5 19 ,2

Quantities so nearly equal, prove the uniform variation of the diff. of N. P. D. of these stars.

(2) α Pegasi and α Serpentis.

	Greenwich. 1755 decl.		Dodagoontah. 1805, Z. D.		Dublin. 1823, N.P.D.
α Serpentis	7° 12′ 48″,6		5° 56′ 59″,6		83° 0′ 36″,6
α Pegasi -	13 53 29 ,7		1 9 37 ,8		75 44 41 ,3
	6 40 41 ,1		7 6 37 ,4		7 15 55 ,3
			6 40 41 ,4		7 6 37 ,4
		(509)	25 56 ,3	(189)	9 17 ,9

Secular variation of annual precession in diff. decl. = + ,349 − ,116 = + ,233.

Hence,

Rate at 1780 } in ten years { 5′ 11″,3 } reduced to 1780 { 5′ 11″,3
at 1814 } { 5 9 9 } { 5 10 ,7

From these small differences we cannot conclude a southern motion in these stars when compared with α Serpentis. Mr. POND's observations made it, in both γ and α Pegasi, upwards of 2″.

(V.) Sirius has, according to Mr. POND, a greater southern motion than any other star, amounting to 3″,4 for ten years, between 1813 and 1823, compared with its rate for ten years at 1784.

This star, in these latitudes, is far from the zenith, on which account, the result of the observations of M. PIAZZI,

at Palermo, will be of considerable authority in estimating the value of observations made at Greenwich, and in Dublin.

	N.P.D. Sirius.
The Cat of M. PIAZZI, gives, Jan. 1, 1800	106° 27′ 6″,2
Red. 13 y. (ann. var. 4″,40)	+ 57 ,2
Jan. 1, 1813	106 28 3 ,4
Red. 10 y. (ann. var. 4″,44)	44 ,4
	106 28 47 ,8

Hence,

	1813.	1823.
Computed from Palermo Cat.	106° 28′ 3″,4	106° 28′ 47″,8
Greenwich Cat.	0 ,7	48 ,7
Dublin Cat.	4 ,3	48 ,4

There can therefore be little doubt, that the apparent southern motion of this star at Greenwich, has arisen principally from an error in the result of the Greenwich observations of 1813.

(VI.) Several of the stars of M. PIAZZI's Catalogue have been already referred to in this enquiry. It is right to remark, also, the general agreement of the Dublin Catalogue of 1813, and the N. P. D. distances for 1813, deduced from M. PIAZZI's Catalogue, taking the annual variations, (reduced to 1806) that were obtained by a comparison of BRADLEY's Catalogue of 1755, by BESSEL, with the Dublin Catalogue of 1823. These variations are given in Table V, column 5.

In this Table, in Column 1, will be found how much the respective stars of the Dublin Catalogue of 1813 are north or south of their places so computed (predicted) from the Palermo Catalogue.

It is evident here is no southern motion, the mean of all

the differences is 0″,1 north, a remarkable confirmation of the exactness of the annual variations used.

Column 2, of Table V., shows how much the observed places, 1823, are north or south of their places, computed from the Catalogue of 1813. These results are mentioned before, and are only placed here to be seen at one view with the rest.

It has been supposed, that Mr. POND's Westbury observations afford a confirmation of the southern motion. Column 3, of Table V., shows how much the observed places, at Greenwich, 1813, are north or south of the places predicted from the Westbury Catalogue.

Column 4 contains Mr. POND's differences between his Catalogue, 1823, and the places predicted from his Catalogue, 1813. A comparison of Columns 3 and 4 will show, that the Westbury Catalogue is, in many instances, so irregularly at variance with the Greenwich Catalogues of 1813 and 1823, that no conclusion whatever can be deduced from it.

In the Conn. des Tems. 1809, p. 458, are given declinations of four stars observed by MECHAIN, with the repeating circle of BORDA, which, at first sight, may appear to support the southern motion.

The first of these stars is Capella, N.P.D. 1800, 44° 13′ 18″,0. The zenith distance of this star, observed at Greenwich by General MUDGE with the zenith sector, and reduced to 1800, is 5° 41′ 51″,0 + 9′,2. Hence the co-lat. of Greenwich = 38° 31′ 17″,8. Therefore, either the zenith sector, or BORDA's circle, must have been in error; and had we not proof of the exactness of the sector, we could scarcely hesitate between the two instruments.

Two of the four stars above mentioned are β Tauri and Pollux. MECHAIN's declination gives the predicted N. P. D. of β Tauri, conformable to Mr. POND's southern motion, who makes it only 0″,7, a quantity evidently too small to found an argument on; besides, MECHAIN's places of this star, deduced at Paris and Montjoy, differ by 1″,8.

Pollux also gives a southern motion, but Mr. POND, finds the southern motion of this star only 0″,2. Also MECHAIN's results as to Pollux are very discordant.

MECHAIN's declination of Sirius also seems to support the southern motion, but in this it is opposed by that of PIAZZI.

I shall conclude by mentioning a result recently obtained, that shows, in a remarkable manner, that the Dublin circle has been consistent with itself from the beginning, and has suffered no derangement.

From 1809 to 1823, inclusive, thirteen summer solstices have been observed with the circle, for which, observations on eighty seven days have been made. I have investigated from these, the maximum of *lunar nutation*, and found it = 9″,60, which happens to be exactly what I have hitherto used for the Sun. I am induced, however, to give more weight to my result from the stars, viz. 9″,26*. The difference is less than could have been expected from solar observations. It puts beyond all doubt the permanent state of the instrument. Had any circumstances taken place similar to those, of whatever nature they may be, by which the Greenwich instrument has shown so great a southern motion in certain stars, they must have given a very erroneous quantity of lunar nutation.

* Philosophical Transactions, 1821.

POSTSCRIPT.

Since the above was written, the kindness of a friend has communicated to me, by letter, Mr. Pond's Paper, read in June last, and which has appeared in the Second Part of the Transactions recently published: the volume itself has not reached me, and therefore I have not seen the Tables. I find Mr. Pond has referred to the Palermo Catalogue, as contained in the Philosophical Transactions, 1806. That Catalogue has been long rejected by the author. The improved places of the principal stars, as given in the great Catalogue, are those to which I have referred. This explanation appears necessary.

The exact Catalogue was first published, probably about 1807, as the Conn. des Tems. 1809, p. 458, which was published in 1807, contains the principal stars agreeing with the great Catalogue very nearly. The observations were therefore made prior to 1807. Indeed it is probable both Catalogues were founded on nearly the same observations. I beg leave to refer here to Mr. Bessel's "Astron. Fundam. p. 297 and 298," for some remarks relative to the improved Catalogue of M. Piazzi.

Mr. Pond states, that unless the southern motion be admitted, the Greenwich observations of 1813 will appear very erroneous, and those of Dublin still more so. As I am unacquainted with the arguments by which he supports this opinion, I cannot reply to them. But I think quite the contrary, as far as regards my observations, will appear from the preceding pages. The southern motion will change every

thing in my results, as well as in those of other astronomers; whereas, without it, every thing is consistent; and I cannot but feel considerable satisfaction in the conviction, that, sidereal astronomy is a more certain science than it is represented to be in the last communication of Mr. POND to the Royal Society.

Observatory, Trinity College, Dublin,
Dec. 6, 1823.

TABLE I.

	Greenwich Catalogue. N. P. D. 1813.	Dublin Catalogue. N. P. D. 1813.	Diff.	Greenwich Catalogue. N. P. D. 1823.	Dublin Catalogue. N. P. D. 1823.	Diff.
	° ′ ″	° ′ ″		° ′ ″	° ′ ″	″
Polaris	1 41 21,6	1 41 21,8	+0,2	1 38 7,5	1 38 7,3	—0,2
β Ursæ Min.	15 4 49,0	15 4 49,4	+0,4	15 7 15,7	15 7 16,7	+1,0
β Cephei	20 15 30,6	20 15 31,4	+0,8	20 12 54,0	20 12 54,4	+0,4
α Ursæ Maj.	27 14 31,5	27 14 30,9	—0,6	27 17 43,7	27 17 44,0	+0,3
α Cephei	28 12 12,5	28 12 13,9	+1,4	28 9 42,8	28 9 42,7	—0,1
α Cassiopeæ	34 29 22,7	34 29 22,6	—0,1	34 26 5,7	34 26 4,1	—1,6
γ Ursæ Maj.	35 15 55,3	35 15 56,2	+0,9	35 19 14,8	35 19 15,1	+0,3
γ Draconis	38 29 3,7	38 29 3,7	0,0	38 29 10,5	38 29 10,3	—0,2
η Ursæ Maj.	39 44 57,9	39 44 58,4	+0,5	39 47 59,4	39 47 59,9	+0,5
α Persei	40 48 52,6	40 48 51,4	—1,2	40 46 39,2	40 46 37,9	—1,3
Capella	44 12 20,5	44 12 20,7	+0,2	44 11 36,8	44 11 36,2	—0,6
α Cygni	45 22 57,0	45 22 58,3	+1,3	45 20 52,4	45 20 52,0	—0,4
α Lyræ	51 23 0,5	51 23 0,8	+0,3	51 22 31,2	51 22 30,8	—0,4
Castor	57 42 46,7	57 42 47,5	+0,8	57 43 59,0	57 43 58,8	—0,2
Pollux	61 31 56,4	61 31 56,1	—0,3	61 33 17,0	61 33 17,2	+0,2
β Tauri	61 33 43,7	61 33 44,2	+0,5	61 33 6,5	61 33 6,7	+0,2
α Andromedæ	61 56 29,6	61 56 30,3	+0,7	61 53 12,5	61 53 12,0	—0,5
α Cor. Bor.	62 38 55,4	62 38 55,5	+0,1	62 41 0,6	62 41 0,3	— 0,3
α Arietis	67 25 36,5	67 25 36,8	+0,3	67 22 44,4	67 22 43,7	—0,7
Arcturus	69 50 19,0	69 50 19,3	+0,3	69 53 29,2	69 53 29,6	+0,4
Aldebaran	73 52 35,4	73 52 36,0	+0,6	73 51 17,7	73 51 17,6	—0,1
β Leonis	74 22 57,3	74 22 56,4	—0,9	74 26 18,1	74 26 17,9	—0,2
α Herculis	75 23 14,0	75 23 14,6	+0,6	75 24 0,1	75 24 0,5	+0,4
α Pegasi	75 47 51,6	75 47 52,8	+1,2	75 44 41,8	75 44 41,3	—0,5
γ Pegasi	75 51 21,0	75 51 21,2	+0,2	75 48 2,4	75 48 0,4	—2,0
Regulus	77 7 22,7	77 7 23,1	+0,4	77 10 15,6	77 10 16,7	+1,1
α Ophiuchi	77 17 39,1	77 17 40,5	+1,4	77 18 10,6	77 18 10,5	—0,1
γ Aquilæ	79 50 0,6	79 50 1,3	+0,7		79 48 37,9	
α	81 36 58,8	81 36 59,8	+1,0	81 35 29,5	81 35 28,9	—0,6
α Orionis	82 38 15,7	82 38 15,9	+0,2	82 38 4,2	82 38 4,0	—0,2
α Serpentis	82 58 39,3	82 58 38,8	—0,5	83 0 36,6	83 0 36,6	0,0
β Aquilæ	84 3 4,1	84 3 5,2	+1,1		84 1 38,9	
Procyon	84 18 14,4	84 18 15,3	+0,9	84 19 43,3	84 19 43,0	—0,3
α Ceti	86 39 0,7	86 39 2,0	+1,3	86 36 36,8	86 36 36,2	—0,6
α Aquarii	91 13 21,6	91 13 21,7	+0,1	91 10 31,4	91 10 30,3	—1,1
α Hydræ	97 51 11,3	97 51 11,0	—0,3	97 53 44,5	97 53 44,2	—0,3
Rigel	98 25 33,8	98 25 34,3	+0,5	98 24 48,4	98 24 48,1	—0,3
Spica Virginis	100 10 51,3	100 10 51,3	0,0	100 14 0,7	100 14 2,0	+1,3
1 α Capricorni	103 4 35,4	103 4 36,1	+0,7	103 2 49,6	103 2 49,7	+0,1
2	103 6 52,3	103 6 52,0	—0,3	103 5 6,6	103 5 7,0	+0,4
2 α Libræ	105 15 22,7	105 15 22,6	—0,1	105 17 56,3	105 17 58,3	+2,0
Sirius	106 28 0,7	106 28 4,3	+3,6	106 28 48,7	106 28 48,4	—0,3
Antares	116 0 16,6	116 0 16,8	+0,2	116 1 44,1	116 1 44,1	0,0

TABLE II.

	N. P. D. Dublin. Jan. 1, 1822.	Zenith Distance. Greenwich. Jan. 1, 1822.	Co-Latitude of Greenwich.
	° ′ ″	° ′ ″	° ′ ″
β Ursæ Min.	15 7 1,9	23 24 20,0	38 31 21,9
β Cephei	20 13 9,9	18 18 11,3	21,2
α Ursæ Maj.	27 17 24,7	11 13 56,6	21,3
α Cephei	28 9 57,7	9 21 23,1	20,8
α Cassiopeæ	34 26 24,0	4 4 55,0	19,0
Capella	44 11 40,5	5 40 20,4	20,1
α Cygni	45 21 4,4	6 49 44,1	20,3
α Lyræ	51 21 33,8	12 50 13,0	20,8
Castor	57 43 51,5	19 12 31,0	20,5
Pollux	61 33 9,0	23 1 47,8	21,2
β Tauri	61 33 10,3	23 1 49,3	21,0
α Andromedæ	61 53 31,9	23 22 11,6	20,3
α Cor. Bor.	62 39 48,1	24 8 27,1	21,0
α Arietis	67 23 1,1	28 51 40,8	20,3
Arcturus	67 53 10,5	31 21 49,2	21,3
Aldebaran	73 51 25,5	35 20 4,5	21,0
β Leonis	74 25 57,9	42 54 37,1	20,8
α Herculis	75 23 55,9	36 52 34,6	21,3
α Pegasi	75 45 0,5	37 13 40,1	20,4
Regulus	77 9 59,3	38 38 37,5	21,8
α Ophiuchi	77 18 7,5	38 46 46,8	20,7
α Aquilæ	81 35 38,0	43 4 17,6	20,4
α Orionis	82 38 5,2	44 6 44,6	20,6
α Serpentis	83 0 24,9	44 29 3,7	21,2
Procyon	84 19 17,0	45 48 13,6	20,7
α Aquarii	91 11 22,0	52 39 27,7	19,8
α Hydræ	97 52 58,5	59 22 8,4	20,5
Spica Virg.	100 3 43,0	61 32 20,8	22,2
Sirius	106 28 43,9	67 57 23,3	20,6
Antares	116 1 35,6	77 30 14,4	21,2

TABLE III.

Annual Variation. 1818.	Secular Variation. 1718. = 1818.		From recent Observations, Dublin Cat. for 1823.	Computed from Cat. 1813.	Diff.
″	″		° ′ ″	″	″
— 20,077	— 0,013	γ Pegasi	75 48 0,4	0,4	0,0
19,866	— 0,067	α Cassiop.	34 26 4,1	3,9	+ 0,2
19,452	— —	Polaris	1 38 7,3	7,3	0,0
17,382	— 0,240	α Arietis	67 22 43,7	42,9	+ 0,8
14,536	— 0,316	α Ceti	86 36 36,2	36,7	— 0,5
13,430	— 0,456	α Persei	40 46 37,9	37,1	+ 0,8
7,889	— 0,458	Aldebaran	73 51 17,6	17,1	+ 0,5
4,507	— 0,622	Capella	44 11 36,2	35,6	+ 0,5
4,690	— 0,411	Rigel	98 24 48,1	47,4	+ 0,7
3,749	— 0,540	β Tauri	61 33 6,7	6,7	0,0
— 1,299	— 0,473	α Orionis	82 38 4,0	3,0	+ 1,0
+ 4,444	— 0,380	Sirius	106 28 48,4	48,7	— 0,3
7,157	— 0,527	Castor	57 43 58,8	59,1	— 0,3
8,687	— 0,422	Procyon	84 19 43,1	42,2	+ 0,9
8,054	— 0,491	Pollux	61 33 17,2	16,6	+ 0,6
15,230	— 0,273	α Hydræ	97 53 44,2	43,3	+ 0,9
17,281	— 0,233	Regulus	77 10 16,7	15,9	+ 0,8
19,122	— 0,160	β Urs. Maj.	32 40 16,1	15,9	+ 0,2
19,272	— 0,160	α	27 17 44,0	43,6	+ 0,4
20,049	— 0,036	β Leonis	74 26 17,9	16,9	+ 1,0
20,001	— 0,029	γ Urs. Maj.	35 19 15,2	16,2	— 1,0
19,700	+ 0,084	α	33 4 38,2	39,1	— 0,9
18,989	+ 0,153	Spica Virg.	100 14 2,0	1,2	+ 0,8
18,960	+ 0,122	ζ Ursæ Maj.	34 8 50,4	52,3	— 1,9
18,173	+ 0,153	η	39 47 59,9	0,1	— 0,2
18,984	+ 0,216	Arcturus	69 53 29,6	29,2	+ 0,4
15,335	+ 0,313	2 α Libræ	105 17 58,3	55,9	+ 2,4
14,765	— 0,029	β Ursæ Min.	15 7 16,7	17,1	— 0,4
12,449	+ 0,295	α Cor. Bor.	62 41 0,3	0,0	+ 0,3
11,740	+ 0,349	α Serpentis	83 0 36,6	36,2	+ 0,4
8,580	+ 0,484	Antares	116 1 44,1	42,6	+ 1,5
4,577	+ 0,387	α Herculis	75 24 0,5	0,4	+ 0,1
3,086	+ 0,400	α Ophiuchi	77 18 10,6	11,4	— 0,8
+ 0,689	+ 0,202	γ Draconis	38 29 10,3	10,6	— 0,3
— 2,993	+ 0,291	α Lyræ	51 22 30,8	30,8	— 0,0
8,326	+ 0,376	γ Aquilæ	79 48 37,9	38,1	— 0,2
9,044	+ 0,384	α	81 35 28,9	29,4	— 0,5
8,555	+ 0,369	β	84 1 38,9	39,7	— 0,8
10,634	+ 0,411	1 α Capric.	103 2 49,7	49,7	0,0
10,669	+ 0,411	2 α Capric.	103 5 7,0	5,3	+ 1,7
12,585	+ 0,227	α Cygni	45 20 52,0	52,5	— 0,5
15,037	+ 0,131	α Cephei	28 9 42,7	43,5	— 0,8
15,656	+ 0,064	β Cephei	20 12 54,4	54,8	— 0,4
17,244	+ 0,227	α Aquarii	91 10 30,3	29,3	+ 1,0
19,285	+ 0,116	α Pegasi	75 44 41,3	40,0	+ 1,3
19,932	— 0,004	α Andromedæ	61 53 12,0	11,0	+ 1,0

TABLE IV.

	Ann. Var. 1820. Mr. Pond.	Ann. Var. 1820. Dr. Brinkley.	Ann. Var. 1820. Mr. Bessel.
	″	″	″
γ Pegasi	− 20,09	− 20,08	− 20,03
α Cassiopeæ	19,85	19,87	—
α Arietis	17,40	17,38	17,35
α Ceti	14,59	14,53	14,49
α Persei	13,41	13,43	—
Aldebaran	7,92	7,88	7,86
Capella	4,54	4,50	4,48
Rigel	4,74	4,69	4,66
β Tauri	3,80	3,74	3,71
α Orionis	− 1,36	− 1,29	− 1,27
Sirius.	+ 4,41	+ 4,45	+ 4,48
Castor	7,12	7,16	7,19
Procyon	8,63	8,69	8,74
Pollux	8,02	8,06	8,09
α Hydræ	15,19	15,23	15,27
Regulus	17,23	17,29	17,31
α Ursæ Maj.	19,26	19,27	—
β Leonis	20,04	20,05	20,08
γ Ursæ Maj.	19,98	20,00	—
Spica Virg.	18,94	18,99	19,03
η Ursæ Maj.	18,15	18,17	—
Arcturus	18,97	18,98	19,01
1 α Libræ	15,30	—	15,40
2 α Libræ	15,32	15,33	15,37
β Ursæ Min.	14,74	14,76	—
α Cor. Bor.	12,45	12,44	12,48
α Serpentis	11,72	11,73	11,79
Antares	8,59	8,58	8,65
α Herculis	4,57	4,57	4,61
α Ophiuchi	3,08	3 08	3,12
γ Draconis	+ 0,67	+ 0,68	—
α Lyræ	− 3,02	− 3,00	− 2,96
γ Aquilæ	8,34	8,33	8,29
α Aquilæ	9,06	9,05	9,00
β Aquilæ	8,56	8,56	8,49
1 Capricorni	10,66	10,64	10,58
2 Capricorni	10,68	10,68	10,61
α Cygni	12,63	12,59	12,56
α Cephei	15,07	15,04	—
β Cephei	15,68	15,66	—
α Aquarii	17,27	17,25	17,20
α Pegasi	19,32	19,28	19,26
α Andromedæ	− 19,95	− 19,93	19,91

TABLE V.

	Diff. of Palermo Cat. 1800, and Dublin, 1813.	Diff. of Dublin Cat. 1813, and Dublin, 1823.	Diff. of Westbury Cat. 1800, and Greenwich, 1813.	Diff. of Greenwich Cat. 1813, and Greenwich, 1823.	Annual Variations, 1806.
	"	"	"	"	"
γ Pegasi	1,2 N	0,1 N	5,2 S	2,3 S	— 20,080
α Cassiopeæ	1,4 N	0,1 S		1,5 S	19,875
α Arietis	0,3 N	0,8 S	2,4 S	1,8 S	17,412
α Ceti	0,3 N	0,5 N	0,0	2,0 S	14,579
α Persei	0,2 N	0,8 S		0,9 S	13,494
Aldebaran	1,3 S	0,5 S	3,4 S	1,5 S	7,944
Capella	2,6 N	0,5 S	1,4 S	1,7 S	4,582
Rigel	0,4 N	0,7 S	0,8 N	2,0 S	4,739
β Tauri	0,7 N	0,0	0,2 N	0,8 S	3,814
α Orionis	1,5 N	1,0 S	2,0 S	2,1 S	— 1,356
Sirius	0,9 S	0,3 N	1,7 S	3,4 S	+ 4,399
Castor	0,0	0,3 N	0,4 S	0,9 S	7,114
Procyon	1,5 S	0,8 S	0,8 S	2,9 S	8,636
Pollux	0,5 N	0,6 S	0,5 N	0,2 S	7,996
α Hydræ	1,0 N	0,9 S	0,8 S	1,3 S	15,191
Regulus	0,8 S	0,8 S	4,6 S	0,6 S	17,254
β Ursæ Maj.	0,9 S	0,1 S			19,104
α	1,0 N	0,4 S		0,5 N	19,254
β Leonis	0,5 S	1,0 S	4,4 S	0,4 S	20,046
γ Ursæ Maj.	0,8 N	1,1 N		0,4 N	19,998
ε	1,7 N	0,8 N			19,710
Spica Virginis	0,2 S	0,8 S	1,4 S	0,0	19,008
ζ Ursæ Maj.	1,8 S	1,9 N			18,974
η	1,1 S	0,2 N		0,2 N	18,192
Arcturus	0,5 S	0,4 S	4,6 S	0,4 S	19,010
2 α Libræ	1,3 N	2,4 S		0,6 S	15,376
β Ursæ Min.	2,1 N	0,4 N		0,7 N	14,762
α Cor. Bor.	1,2 S	0,3 S	0,3 S	0,5 S	12,485
α Serpentis	0,7 N	0,4 S	4,1 S	0,1 S	11,782
Antares	1,6 N	1,5 S		1,6 S	8,645
α Herculis	2,2 S	0,1 S		0,4 S	4,627
α Ophiuchi	2,3 S	0,8 N	4,6 S	0,9 S	3,134
γ Draconis	1,1 N	0,3 N	0,8 S	0,2 S	+ 0,713
α Lyræ	0,1 S	0,0	3,2 S	0,8 S	— 2,959
γ Aquilæ	0,3 S	0,2 N			8,281
α	2,0 S	0,5 N	4,5 S	1,2 S	8,999
β	1,0 S	0,8 N			8,510
1 α Capricorni	2,2 S	0,1 N		1,0 S	10,586
2	0,1 N	1,7 S	2,6 S	1,0 S	10,620
α Cygni	1,3 S	0,5 N	3,4 S	1,6 S	12,558
α Cephei	2,0 N	0,8 N		0,6 S	15,022
β	2,4 N	0,5 N		0,2 S	15,647
α Aquarii	0,6 N	1,0 S	0,8 S	2,5 S	17,219
α Pegasi	0,4 S	1,4 S	4,3 S	3,3 S	19,268
α Andromedæ	1,6 N	1,0 S	1,1 N	1,8 S	19,933

V. *On the figure requisite to maintain the equilibrium of a homogeneous fluid mass that revolves upon an axis.* By James Ivory, *A. M. F. R. S.*

Read December 18, 1824.

The theory of the figure of the earth, as delivered in the *Philosophiæ Naturalis Principia Mathematica*, is liable to some objections. In determining the ratio of the axes, the illustrious author assumes that the terrestrial meridian is an ellipse, having the greatest diameter in the plane of the equator. M'Laurin afterwards proved, by a most elegant synthetic process of reasoning, that a homogeneous fluid body, possessed of such a figure as Newton supposed, will fulfil all the conditions of equilibrium arising from the attraction of the particles, and a centrifugal force of rotation. In this manner the assumption of Newton was verified; but the theory was still left imperfect, since it is necessary to determine, by a direct investigation, all the figures of a fluid mass that are consistent with the laws of equilibrium, rather than to show that the same laws will be fulfilled in particular instances. We are indebted to Legendre for the first demonstration that a homogeneous fluid body, revolving about an axis, cannot be in equilibrio by the attraction of its particles, unless it have the figure of an oblate elliptical spheroid. The researches of Legendre were rendered more general by Laplace, who gave a complete theory of the figure of the planets, distinguished by that depth and elegance which is so much admired in all his writings. It is assumed, however

by the eminent geometers we have mentioned, that the figure of the fluid mass is but little different from a sphere which is a restriction not essential to the problem, but introduced for the sake of overcoming some of the difficulties of the investigation. In the following Paper, the figure of a homogeneous fluid body, that revolves about an axis, and is in equilibrio by the attraction of its particles, is deduced by a direct analysis in which no arbitrary supposition is admitted.

1. It is necessary to begin this research, with laying down some general properties of the attractions of bodies ; and we cannot better accomplish this end, than by considering the function, which is the sum of all the molecules of a body divided by their respective distances from the attracted point. Conceive any material body to be divided into an indefinitely great number of molecules, one of which is represented by dm; and having drawn three planes intersecting at right angles within the body, let x, y, z, denote the co-ordinates that determine the position of dm, and a, b, c, those that determine the attracted point: then, if we put

$$r = \sqrt{a^2 + b^2 + c^2}$$
$$f = \sqrt{(a-x)^2 + (b-y)^2 + (c-z)^2};$$

r will be the distance of the attracted point from the origin of the co-ordinates, and f that of dm from the attracted point.

$$\text{Now let } V(r) = \int \frac{dm}{f},$$

the sign of integration, extending to all the molecules of the body: and $V(r)$ will be the function alluded to, and which we have to consider.

It need not be mentioned that $V(r)$ is not a function of r, but of the three co-ordinates a, b, c; or it is an abridged

symbol denoting a function of r, and the angles which that line makes with the axes of the co-ordinates.

The distinguishing property of the expression $V(r)$ is this: if we take its fluxions with respect to the variable quantities a, b, c, the differential coefficients $-\left(\frac{d.V(r)}{da}\right)$, $-\left(\frac{d.V(r)}{db}\right)$, $-\left(\frac{d.V(r)}{dc}\right)$, will be respectively equal to the accumulated attractions of all the molecules of the body on the attracted point in the directions of a, b, c, and tending to shorten these lines.

Suppose now another body similar to the first in its lineal dimensions, and likewise having the parts similarly situated of the same density. If, therefore, this second body be divided into the same number of similar molecules as the first body; every two molecules, dm and dm', situated alike, will be of equal density, and their volumes will be proportional to the volumes of the two whole bodies. Suppose, also, that x', y', z', are three rectangular co-ordinates of the molecule dm', drawn to planes situated in the second body, similarly to the like planes in the first; and farther, let a', b', c', be the co-ordinates of an attracted point, placed in the same relative situation in the second body, as the former attracted point in the first; then,

$$r' = \sqrt{a'^2 + b'^2 + c'^2}$$
$$f' = \sqrt{(a' - x')^2 + (b' - y')^2 + (c' - z')^2}$$
$$V'(r') = \int \frac{dm'}{f'}.$$

It is manifest from what has been said, that r and r', f and f', are homologous lines of the two bodies; and hence,

$$\frac{r}{f} = \frac{r'}{f'}; \quad \frac{dm}{r^3} = \frac{dm'}{r'^3}:$$

consequently,

$$\frac{1}{r^2} \cdot \frac{dm}{f} = \frac{1}{r'^2} \cdot \frac{dm'}{f'}:$$

and as this is universally true of all the molecules, we have,

$$\frac{V(r)}{r^2} = \frac{V'(r')}{r'^2}.$$

In this form the expression is inconvenient, because both the quantities become infinite when we suppose that the attracted point is placed in the origin of the co-ordinates. But the inconvenience is easily removed by substituting, for r and r', any other two homologous lines. Let r and r', produced if necessary, meet the surfaces; and let R and R′ denote the parts within the two bodies, intercepted between the surface and the origin of the co-ordinates: then it is manifest that $\frac{R}{r} = \frac{R'}{r'}$; and we shall therefore have

$$\frac{V(r)}{R^2} = \frac{V'(r')}{R'^2}.$$

If, therefore, we suppose a series of such bodies as we have been describing, which increase in magnitude from zero to infinity, while they constantly preserve the same proportions in their lineal dimensions, and the same densities of the parts similarly situated; the quantity $\frac{V(r)}{R^2}$, will have constantly the same value in all the bodies, supposing that the attracted points are alike placed in them all. It is manifest, therefore, that $\frac{V(r)}{R^2}$ will depend only on what is common to all the bodies in the series; or it will be a function of the quantities that remain unchanged in passing from one of the bodies to another. But as these quantities are not the same in all positions of the attracted point, it will be proper to distinguish several cases.

First, let the attracted point be in the surface, in which case $r = \mathrm{R}$: then, $\frac{a}{\mathrm{R}}, \frac{b}{\mathrm{R}}, \frac{c}{\mathrm{R}}$ are the only quantities that constantly retain the same values in all the bodies. These quantities remain unchanged, because the line r, or R, always makes the same angles with the axes of the co-ordinates. We therefore have,

$$\frac{\mathrm{V}(\mathrm{R})}{\mathrm{R}^2} = \mathrm{F}.\left\{\frac{a}{\mathrm{R}}, \frac{b}{\mathrm{R}}, \frac{c}{\mathrm{R}}\right\},$$

the letter F being the mark of a function. Hence,

$$\mathrm{V}(\mathrm{R}) = \mathrm{R}^2 \times \mathrm{F}.\left\{\frac{a}{\mathrm{R}}, \frac{b}{\mathrm{R}}, \frac{c}{\mathrm{R}}\right\}.$$

Again, let us put,

$$a = r.\mu;\; b = r\sqrt{1-\mu^2}.\,\mathrm{Cos.}\,\varpi;\; c = \gamma\sqrt{1-\mu^2}.\,\mathrm{Sin.}\,\varpi;$$

and μ will be the cosine of the angle which the line r, or R, makes with the axis of a; and ϖ the angle which the projection of the same line upon the plane of b and c makes with the axis of b: then, when $r = \mathrm{R}$, we have,

$$\mathrm{V}(\mathrm{R}) = \mathrm{R}^2 \times \mathrm{F}.\left\{\mu, \sqrt{1-\mu^2}.\,\mathrm{Cos.}\,\varpi, \sqrt{1-\mu^2}.\,\mathrm{Sin.}\,\mu\right\}.$$

Secondly, suppose that the attracted point is placed within each of the bodies; then the quantities common to them all are these, viz. $\frac{r}{\mathrm{R}}, \frac{a}{r}, \frac{b}{r}, \frac{c}{r}$. Hence,

$$\frac{\mathrm{V}(r)}{\mathrm{R}^2} = \mathrm{F}.\left\{\frac{r}{\mathrm{R}}, \frac{a}{r}, \frac{b}{r}, \frac{c}{r}\right\}.$$

Consequently,

$$\mathrm{V}(r) = \mathrm{R}^2 \times \mathrm{F}.\left\{\frac{r}{\mathrm{R}}, \frac{a}{r}, \frac{b}{r}, \frac{c}{r}\right\},$$

$$\mathrm{V}(r) = \mathrm{R}^2 \times \mathrm{F}.\left\{\frac{r}{\mathrm{R}}, \mu, \sqrt{1-\mu^2}.\,\mathrm{Cos.}\,\varpi, \sqrt{1-\mu^2}.\,\mathrm{Sin.}\,\varpi\right\}.$$

In order to have a more exact notion of this function, we may suppose it to be expanded in a series of the powers of the fraction $\frac{r}{\mathrm{R}}$: then,

$$V(r) = R^2 \times \left\{ P^{\circ} + P^{(1)} . \frac{r}{R} + P^{(2)} . \frac{r^2}{R^2} + \&c. \right\},$$

the coefficients P°, $P^{(1)}$, $P^{(2)}$, &c. being all functions of μ, $\sqrt{1-\mu^2}$. Cos. ϖ, $\sqrt{1-\mu^2}$. Sin. ϖ. When the attracted point coincides with the origin of the co-ordinates, the value of $\frac{V(o)}{R^2}$ is equal to $P^{(o)}$; and when the same point is in the surface, then $\frac{r}{R} = 1$, and the value of $\frac{V(R)}{R^2}$ is equal to

$$P^{(o)} + P^{(1)} + P^{(2)} + \&c.$$

Finally, let the attracted point be without the surface; then the quantities common to all the bodies are these, viz. $\frac{R}{r}$, $\frac{a}{r}$, $\frac{b}{r}$, $\frac{c}{r}$: hence

$$\frac{V(r)}{R^2} = F . \left\{ \frac{R}{r}, \frac{a}{r}, \frac{b}{r}, \frac{c}{r} \right\}.$$

Consequently,

$$V(r) = R^2 \times F . \left\{ \frac{R}{r}, \frac{a}{r}, \frac{b}{r}, \frac{c}{r} \right\};$$

$$V(r) = R^2 \times F . \left\{ \frac{R}{r}, \mu, \sqrt{1-\mu^2} \text{ Cos. } \varpi, \sqrt{1-\mu^2} . \text{ Sin. } \varpi \right\}.$$

In this case, $\frac{V(r)}{R^2}$ decreases as r increases, and finally vanishes when r is infinite. The expansion must therefore have this form, viz.

$$V(r) = R^2 \times \left\{ Q^{(1)} . \frac{R}{r} + Q^{(2)} . \frac{R^2}{r^2} + Q^{(3)} . \frac{R^3}{r^3} + \&c. \right\},$$

$Q^{(1)}$, $Q^{(2)}$, $Q^{(3)}$, &c. being functions of μ, $\sqrt{1-\mu^2}$. Cos. ϖ, $\sqrt{1-\mu^2}$. Sin. ϖ. When the attracted point is in the surface, $\frac{R}{r^2} = 1$, and the value of $\frac{V(R)}{R^2}$ is equal to

$$Q^{(1)} + Q^{(2)} + Q^{(3)} + \&c.$$

The preceding reasoning is quite general, and will apply

to any material body, whatever be its form. The body may consist of parts not connected by any mathematical law; or, which is the same thing, it is not necessary that the equation of its surface be subject to the law of continuity.

2. The co-ordinates of the molecule dm being x, y, z, let R' denote its distance from the origin of the co-ordinates; and put,

$$x = R'.\mu'; \; y = R\sqrt{1-\mu'^2}.\text{Cos.}\,\varpi'; \; z = R'\sqrt{1-\mu'^2}.\text{Sin.}\,\varpi';$$

then, since we likewise have,

$$a = r.\mu; \; b = r.\sqrt{1-\mu^2}.\text{Cos.}\,\varpi; \; c = r\sqrt{1-\mu^2}.\text{Sin.}\,\varpi;$$

and,

$$f = \sqrt{(a-x)^2 + (b-y)^2 + (c-z)^2};$$

we shall get,

$$\gamma = \mu\mu' + \sqrt{1-\mu^2}.\sqrt{1-\mu'^2}.\text{Cos.}\,(\varpi - \varpi'),$$

$$f = \sqrt{r^2 - 2rR'.\gamma + R'^2}.$$

It now becomes necessary to expand $\frac{1}{f}$ in a series of the powers of $\frac{r}{R}$, or of $\frac{R}{r}$. Much has already been written on this expansion. The coefficients have been exhibited in various forms, and many remarkable properties which they possess have been very diligently explored. It would not, therefore, be necessary to add any thing upon this subject, unless it be possible to give to the same quantities a new and more simple form of expression, useful in the present investigation.

If we suppose,

$$\frac{1}{f} = \frac{1}{r}.\left\{1 + C^{(1)}.\frac{R'}{r} + C^{(2)}.\frac{R'^2}{r} \ldots\ldots + C^{(i)}\frac{R'^i}{r^i} + \&c.\right\};$$

the following differential equation has already been proved in the Philosophical Transactions for 1812, viz.

$$i(i+1).C^{(i)}+\frac{d.\left\{(1-\gamma^2)\frac{dC^{(i)}}{d\gamma}\right\}}{d\gamma}=0:$$

and from this equation the value of $C^{(i)}$ is deduced, viz.

$$C^{(i)}=\frac{1.3.5\ldots 2i-1}{1.2.3\ldots\ldots i}\times\left\{\gamma^i-\frac{i(i-1)}{2.2i-1}.\gamma^{i-2}+\text{\&c.}\right\}.$$

In the same place another more general differential equation is found, of which the former is only a particular case, viz.

$$(i-n).(i+n+1).(1-\gamma^2)^n\frac{d^nC^{(i)}}{d\gamma^n}+\frac{d.\left\{(1-\gamma^2)^{n+1}\frac{d^{n+1}C^{(i)}}{d\gamma^{n+1}}\right\}}{d\gamma}=$$

For the sake of abridging I shall now put

$$\varphi^{(n)}=(1-\gamma^2)^n.\frac{d^nC^{(i)}}{d\gamma^n};$$

and, consequently,

$$\varphi^{(0)}=(1-\gamma^2)^0\frac{d^0C^{(i)}}{d\gamma^0}=C^{(i)}:$$

then, if in the foregoing formula, we make n successively equal to 0, 1, 2, 3 . . . i, we shall get this series of equations, viz.

$$\varphi^{(0)}+\frac{d.\varphi^{(1)}}{i(i+1.d\gamma}=0$$

$$\varphi^{(1)}+\frac{d.\varphi^{(2)}}{i-1.i+2.d\gamma}=0$$

$$\varphi^{(2)}+\frac{d.\varphi^{(3)}}{i-2.i+3.d\gamma}=0$$

$$\vdots$$

$$\varphi^{(i-1)}+\frac{d.\varphi^{(i)}}{1.2i.d\gamma}=0.$$

Now, by combining these equations, after having taken the fluxions of each a proper number of times, all the intermediate quantities between $\varphi^{(o)}$ and $\varphi^{(i)}$ may be made to disappear; and we shall finally obtain,

$$\varphi^{(o)} - \frac{(-1)^i}{1.2.3\ldots 2i} \times \frac{d^i \varphi^{(i)}}{d\gamma^i} = o:$$

and by restoring the expressions that $\varphi^{(o)}$ and $\varphi^{(i)}$ stand for,

$$C^{(i)} = \frac{(-1)^i}{1.2.3\ldots 2i} \times \frac{d.^i \left\{ (1-\gamma^2)^i \frac{d^i C^{(i)}}{d\gamma^i} \right\}}{d\gamma^i}.$$

But, from the series equal to $C^{(i)}$, we get

$$\frac{d^i C^{(i)}}{d\gamma^i} = 1.3.5\ldots(2i-1).$$

Wherefore,

$$C^{(i)} = \frac{(-1)^i}{2.4.6\ldots 2i} \times \frac{d^i (1-\gamma^2)^i}{d\gamma^i}.$$

From this very simple expression, the most remarkable properties of the coefficients of the expansion of $\frac{1}{f}$, are very readily deduced.

3. We may suppose that the indefinitely small molecule dm is a parallelopiped, of which the height is equal to dR'; and the length to $\frac{R'd\mu}{\sqrt{1-\mu'^2}}$, the small line described by the motion of R' perpendicular to the plane of y, z; and the breadth to $R'd\varpi'\sqrt{1-\mu'^2}$, the small line described by the motion of R' parallel to the same plane. The volume of the molecule is therefore equal to $dR' \times \frac{R'd\mu'}{\sqrt{1-\mu'^2}} \times R'd\varpi'\sqrt{1-\mu'^2}$; and, if ρ be put for its density, we shall have

$$dm = \rho R'^2 dR' d\mu' d\varpi'.$$

Again, we have,

$$f = \sqrt{r^2 - 2rR'\gamma + R'^2},$$
$$V(r) = \int \frac{dm}{f};$$

and, by taking the fluxions with regard to r,

$$-\left(\frac{d \, . \, V(r)}{dr}\right) r = \int \frac{dm\,(r^2 - rR'\gamma)}{f^3}.$$

But,

$$r^2 - rR'\gamma = f^2 + rR'\gamma - R'^2;$$

wherefore,

$$-\left(\frac{d \, . \, V(r)}{dr}\right) r = \int \frac{dm}{f} + \int \frac{dm\,(rR'\gamma - R'^2)}{f^3};$$

and, by adding the equivalent quantities, $2\,V(r)$ and $2\int \frac{dm}{f}$, we get

$$2\,V(r) - \left(\frac{d \, . \, V(r)}{dr}\right) r = 3\int \frac{dm}{f} + \int \frac{dm\,(rR'\gamma - R'^2)}{f^3};$$

and by substituting the value of dm,

$$2\,V(r) - \frac{d \, . \, V(r)}{dr}\, r = \iint d\mu'\, d\varpi' \, . \int \varrho \, . \left\{ \frac{3R'^2 dR'}{f} + \frac{R'^3 (r\gamma - R')\, dR'}{f^3} \right\}$$

or, which is the same thing,

$$-\left(\frac{d \, . \, \frac{V(r)}{r^2}}{dr}\right) r^3 = \iint d\mu'\, d\varpi' \, . \int \left(\frac{d \, . \, \frac{R'^3}{f}}{dR'}\right) \varrho \, dR'.$$

In the present paper, we confine our attention to a homogeneous body, or fluid mass; and, ϱ being constant, we may suppose it equal to unit; then, having integrated the last equation with regard to the variable R', we shall get,

$$-\left(\frac{d \, . \, \frac{V(r)}{r^2}}{dr}\right) r^3 = \iint \frac{R'^3 \, d\mu' \, d\varpi'}{f}.$$

In this formula, R' is the line drawn from the origin of the co-ordinates to the surface; and the integration with respect to μ', is to extend from $\mu' = 1$ to $\mu' = -1$; and that with

respect to ϖ', from $\varpi' = o$ to $\varpi' = 2\pi$, or the whole circumference.

The preceding formula is true, whether the attracted point be without or within the body. There is however a distinction between the two cases. If we multiply by $-\frac{dr}{r^4}$, and then integrate, we shall get

$$\frac{V(r)}{r^2} = \int -\frac{dr}{r^3} \iint \frac{R'^3 \, d\mu' \, d\varpi'}{\sqrt{r^2 - 2rR'\gamma + R'^2}},$$

no constant quantity being necessary when the attracted point is without the body, because both the quantities vanish when r is infinitely great. But when the attracted point is within the body, it is necessary to add a constant quantity, because $\frac{dd \,.\, V(r)}{dr^2}$, is not evanescent when $r = o$: in this case, therefore, we have

$$\frac{V(r)}{r^2} = \int -\frac{dr}{r^3} \iint \frac{R'^3 \, d\mu' \, d\varpi'}{\sqrt{r^2 - 2rR'\gamma + R'^2}} + K;$$

and

$$V(r) = r^2 \int -\frac{dr}{r^3} \iint \frac{R'^3 \, d\mu' \, d\varpi'}{\sqrt{r^2 - 2rR'\gamma + R'^2}} + Kr^2,$$

K being a quantity independent of r.

It is necessary to find an expression of the value of K. For this purpose we have,

$$V(r) = \int \frac{dm}{f} = \iiint \frac{R'^2 \, dR' \, d\mu' \, d\varpi'}{\sqrt{r^2 - 2rR'\gamma + R'^2}}.$$

Expand the denominator in a series of the ascending powers of r; then,

$$\begin{aligned} V(r) = & \iiint R' \, dR' \, d\mu' \, d\varpi' \\ & + r \iiint dR' . C^{(1)} \, d\mu' \, d\varpi' \\ & + r^2 \iiint \frac{dR'}{R'} . C^{(2)} \, d\mu' \, d\varpi' \\ & + r^3 \iiint \frac{dR'}{R'^2} . C^{(3)} \, d\mu' \, d\varpi' \\ & + \&c. \end{aligned}$$

The integrations with respect to dR' should be executed from $R' = o$, to the value of R' at the surface of the body; which cannot be done, because all the terms after the two first would be infinite. Conceive a sphere to be described about the origin of the co-ordinates with the radius r; then the whole value of the function $V(r)$ will be equal to its value with respect to the sphere added to its value with respect to the matter between the sphere and the surface of the body. The attracted point being in the surface of the sphere, the first part of the value of $V(r)$ will be equal to $\frac{4\pi}{3} \times r^2$; and the second part will be found by integrating the foregoing expression, so that all the integrals shall vanish when $R' = r$. Thus we get,

$$
\begin{aligned}
V(r) = \frac{4\pi}{3} r^2 &+ \iint \tfrac{1}{2}(R'^2 - r^2)\, d\mu'\, d\varpi' \\
&+ r \iint (R' - r) \,.\, C^{(1)}\, d\mu'\, d\varpi' \\
&+ r^2 \iint (\text{Log.}\, R' - \text{Log.}\, r) \,.\, C^{(2)}\, d\mu'\, d\varpi' \\
&+ \frac{r^3}{1} \iint \left(\frac{1}{r} - \frac{1}{R'}\right) . \, C^{(3)}\, d\mu'\, d\varpi' \\
&+ \frac{r^4}{2} \iint \left(\frac{1}{r^2} - \frac{1}{R'^2}\right) . \, C^{(4)}\, d\mu'\, d\varpi' \\
&+ \text{\&c.}
\end{aligned}
$$

Now the integral $-\frac{1}{2}\iint r^2\, d\mu'\, d\varpi'$, taken between the limits $\mu' = 1$, $\mu = -1$; and $\varpi' = o$, $\varpi' = 2\pi$; is equal to $-2\pi r^2$. All the other parts of the above expression that contain r, are evanescent; because we have generally,

$$\iint C^{(i)}\, d\mu'\, d\varpi' = o.$$

In order to prove this, it is to be observed that μ, μ' and γ are the cosines of the three sides of a spherical triangle, and $\varpi - \varpi'$ is the angle opposite to the side whose cosine is γ:

now if we put ψ for the angle opposite to the side whose cosine is μ', we may write $d\gamma\, d\psi$ in place of $d\mu'\, d\varpi'$, making γ and ψ vary between the same limits as μ' and ϖ'. This is allowable; not that we must conceive the two fluxions as continually equal to one another, but because the total sum, between the prescribed limits, is, in either case, equal to the whole surface of the sphere. If now we substitute the value of $C^{(i)}$ given in § 2, the foregoing expression will become,

$$\frac{(-1)^i}{2.4.6\ldots 2i} \times \iint \frac{d^i.(1-\gamma^2)^i}{d\gamma^i} \times d\gamma \times d\psi :$$

and the integral is,

$$\frac{(-1)^i}{2.4.6\ldots 2i} \times 2\pi \times \frac{d^{i-1}.(1-\gamma^2)^i}{d\gamma^{i-1}};$$

a quantity which, being divisible by $1-\gamma^2$, is evanescent at both the limits of γ.

Omitting what has been proved to be evanescent in $V(r)$, and collecting into one sum all the parts multiplied by r^2, and separating them from the rest, we get

$$\begin{aligned} V(r) = {} & \iint \frac{R'^2}{2}.\, d\mu'\, d\varpi' + r^2 \times \left\{-\frac{2\pi}{3} + \iint \log.R' \times C^{(2)} d\mu'\, d\varpi'\right\} \\ & + r \iint R' C^{(1)} d\mu'\, d\varpi' \\ & - \frac{r^3}{1} \iint \frac{C^{(3)} d\mu'\, d\varpi'}{R'} \\ & - \frac{r^4}{2} \iint \frac{C^{(4)} d\mu'\, d\varpi'}{R'^2} \\ & - \&c. \end{aligned}$$

which expression may be thus written in finite terms, viz.

$$V(r) = r^2 \int -\frac{dr}{r^3} \iint \frac{R'^3\, d\mu'\, d\varpi'}{f} + r^2. \left\{-\frac{2\pi}{3} + \iint \log.R' \times C^{(2)}\, d\mu'\, d\varpi'\right\}$$

as will be evident by expanding $\frac{1}{f}$, and performing the in-

tegrations with respect to dr. By comparing this expression of $V(r)$ with the foregoing one, we get,

$$K = -\frac{2\varpi}{3} + \iint \log. R' \times C^{(2)} \, d\mu' \, d\varpi'.$$

From this expression it follows that K has the same value in all similar homogeneous bodies. Suppose another body, similar and homogeneous to the first, and having the axes of the co-ordinates similarly placed: let R' and $R'_{,}$ be lines drawn to the surface from the origin of the co-ordinates, and making the same angles with the axes; then, K' denoting the like quantity in the second body as K in the first, we have

$$K = -\frac{2\varpi}{3} + \iint \log. R' \times C^{(2)} \, d\mu' \, d\varpi',$$

$$K' = -\frac{2\varpi}{3} + \iint \log. R'_{,} \times C^{(2)} d\mu' \, d\varpi';$$

wherefore,

$$K - K' = \iint \log. \frac{R'}{R'_{,}} \times C^{(2)} d\mu' \, d\varpi'.$$

But the two bodies being similar, and R' and $R'_{,}$ lines similarly drawn in them, it follows that $\frac{R'}{R'_{,}}$ will remain unchanged, when μ' and ϖ' vary. Consequently,

$$K - K' = \log. \frac{R'}{R'_{,}} \times \iint C^{(2)} \, d\mu' \, d\varpi' = 0.$$

4. Having now laid down the properties of attraction to which we shall have occasion to refer, we are next to consider the conditions necessary to the equilibrium of a fluid mass. These were first reduced to a uniform mode of calculation by Clairaut, in his Theory of the Figure of the earth. They are investigated in all the great treatises of rational mechanics; in the *Mecanique Analytique* of Lagrange, the *Mecanique Celeste* of Laplace, and the *Mecanique* of Poisson.

The English reader will likewise find the same investigations in a work published in 1821, under the title of *Elementary Illustrations of the Celestial Mechanics*, which is a translation of the first book of the *Mecanique Celeste*, and, in addition to the text, contains much valuable matter. Referring to these works, we shall first merely enumerate the chief properties of the equilibrium of a fluid mass, for the sake of recalling them to the recollection of the reader; and then make such an application of the general principles as our present purpose requires.

A heterogeneous fluid body cannot be in equilibrio, unless the outer surface be every where of the same density; and farther, unless particles of the same density be arranged in distinct strata in the interior of the mass. The pressure upon all equal spaces of every stratum of uniform density, that is, the force acting perpendicularly to them, and pushing them inward, must be equal. Hence, these are called level strata, or *couches de niveau*, because the direction of the accelerating force, or of gravity, is every where perpendicular to them. It is easy to perceive that the densities must decrease in approaching the outer surface. For, in two contiguous strata of different densities, if we take two molecules equal in volume, and placed at the same point of the separating surface; the common gravity acting upon both will produce a greater pressure in the denser molecule. Wherefore, if the denser matter were nearer the outer surface, it would penetrate into the rarer matter below it; which is contrary to the perfect separation of the strata of different densities.

Supposing all these conditions to be fulfilled, it readily follows that the fluid body will be in equilibrio. For the

equal pressure upon every part of the upper surface of a level stratum, being propagated through the interior fluid, will act with equal force in an opposite direction upon every part of the lower surface; and hence, every molecule of the stratum will be equally pressed in all opposite directions.

When the fluid mass is homogeneous, the distinction of the level strata arising from the difference of their densities is lost; but the possibility of dividing it into any number of strata separated by level surfaces, is still a necessary condition of the equilibrium.

The condition, that every level surface must be a continuous curve stretching through the whole fluid mass, imposes a limitation on the forces with which the equilibrium is possible. All these curve surfaces are defined by a common equation between three independent co-ordinates; and as this equation is to be found by integrating an expression containing the co-ordinates and their fluxions, the operation must be practicable, without supposing any relation between the three variable quantities. Hence, the forces acting on the particles of the fluid must be such, that the three differential coefficients shall fulfil what is called the criterion of integrability; otherwise the equilibrium will be impossible. In determining the equilibrium of the fluid placed on the surfaces of the planets, the nature of the forces brought into action is such, that the problem is always free from contradictory conditions.

To come now to the main object we have in view,* conceive that H K I is a body of homogeneous fluid in equilibrio

* Theorie de la Figure de la Terre par CLAIRAUT. Premiere Partie, cap. V. § XXI.

by the action of all the forces that urge its particles. Let x, y, z, be three rectangular co-ordinates of a point K in the surface; and put X, Y, Z, for the accelerating forces that act upon a particle at K respectively in the directions of the co-ordinates, and tending to diminish these lines. Suppose that K varies its position a little in the fluid's surface; then the condition that the resultant of the forces parallel to the co-ordinates, shall be perpendicular to that surface, is expressed by this equation, viz.

$$X\,dx + Y\,dy + Z\,dz = o.$$

In order that the equilibrium be possible, the expression just set down must be a complete differential; which subjects the forces X, Y, Z, to the criterion of integrability. This condition being fulfilled, the equation of the fluid's surface will be,

$$\int (X\,dx + Y\,dy + Z\,dz) = C,$$

C being an arbitrary constant quantity. If, for the sake of brevity, we represent the preceding integral by φ, we shall have,

$$\varphi = C$$

$$X = \frac{d\varphi}{dx},\ Y = \frac{d\varphi}{dy},\ Z = \frac{d\varphi}{dz}.$$

Again, let

$$p = \sqrt{X^2 + Y^2 + Z^2};$$

then p is the resultant of the forces X, Y, Z; and it acts on a particle placed at K, in a direction perpendicular to the fluid's surface, and tending inward. It is the gravity at that point.

Suppose now that a stratum of fluid is laid upon the surface H K I, the thickness at K being equal to the indefinitely small line K S. The new pressure at K will be proportional

to the superincumbent matter multiplied by p, or by the gravity which urges a particle inward. But, as the density is constant, the quantity of matter pressing at K, will be proportional to the thickness KS. Wherefore, if δC represent the additional pressure at K, we shall have

$$\delta C = p \times KS.$$

Hence,

$$KS = \frac{\delta C}{p} = \frac{\delta C}{\sqrt{X^2 + Y^2 + Z^2}}.$$

If now we suppose that δC remains constant, and, by means of the formula just set down, determine the thickness at every point; it is evident that the stratum will press equably upon the surface of the fluid HKI, and consequently will not disturb the equilibrium by its pressure. It remains to determine the equation of the upper surface of the stratum. For this purpose we have,

$$\delta C = p \times KS = \frac{p^2}{p} \times KS = \left(X\frac{X}{p} + Y\frac{Y}{p} + Z\frac{Z}{p}\right) \times KS.$$

The co-ordinates of the point K being x, y, z, let those of the point s be $x + \delta x, y + \delta y, z + \delta z$: then KS being perpendicular to the surface HKI, it is easy to prove that,

$$\delta x = \frac{X}{\sqrt{X^2 + Y^2 + Z^2}} \times KS = \frac{X}{p} \times KS,$$

$$\delta y = \frac{Y}{p} \times KS$$

$$\delta z = \frac{Z}{p} \times KS;$$

wherefore, by substitution, we get

$$\delta C = X\delta x + Y\delta y + Z\delta z;$$

that is,

$$\delta C = \frac{d\varphi}{dx}\delta x + \frac{d\varphi}{\delta y}\delta y + \frac{d\varphi}{dz}\delta z.$$

Consequently,

$$\varphi + \frac{d\varphi}{dx}\delta x + \frac{d\varphi}{dy}\delta y + \frac{d\varphi}{dz}\delta z = C + \delta C.$$

Now this expression is derived from the equation,

$$\varphi = C,$$

on the one hand, by changing C into $C + \delta C$; and, on the other, by substituting, in the function φ, the co-ordinates $x + \delta x$, $y + \delta y$, $z + \delta z$ of the upper surface of the stratum, in place of x, y, z, the co-ordinates of the surface H K I. Thus it appears, that the equation of the new fluid body is derived from that of the first one, merely by varying the constant introduced in the integration.

Before proceeding farther, it is requisite to distinguish carefully two separate cases. The first is, when the particles of the fluid do not attract one another; and the second, when they are endowed with attractive powers. These are plainly two cases essentially different from one another: for, in the first, a stratum added induces no other change than an increase of pressure; but, in the second, besides the pressure a new force is introduced, arising from the attraction which the matter of the stratum exerts upon the fluid body to which it is added.

In the first case, when there are no new forces introduced by attraction, it is manifest from what has been said, that the fluid body of which the equation is,

$$\varphi = C + \delta C$$

is in equilibrio; because the stratum presses equally upon all parts of the surface H K I. If we suppose a second stratum to be laid upon the first, and compute its thickness by the gravity at the surface N O L, in the same manner that the thickness of the first was determined by means of the gravity at the surface H K I, we shall have another fluid body in equilibrio, of which the equation will be,

$$\phi = C + \delta C + \delta' C$$

$\delta' C$ being equal to the pressure caused by the new stratum. And, in like manner, any number of strata may be added composing a fluid body in equilibrio.

But as strata have been added without disturbing the equilibrium, in like manner any number of strata below the surface H K I, may be successively taken away, so as to leave the remaining fluid in equilibrio. The original body H K I may be thus exhausted, or reduced to an infinitesimal quantity that may be neglected; and then the whole mass, both above and below the surface H K I, will consist of level strata separated by surfaces having a common equation, in which the constant quantity introduced in the integration alone varies in passing from one surface to another. We may therefore conclude that, when the particles of the fluid do not attract one another, the only conditions necessary for the equilibrium are, first, that the force resulting from X, Y, Z, be directed into the interior of the mass; and, secondly, that $X\,dx + Y\,dy + Z\,dz$ be an exact differential.

But this first case can have no application in the theory of the figure of the planets, the leading principle of which is, that every particle of matter attracts every other particle. We must therefore proceed to consider what new conditions are required in the second case, when the particles are possessed of attractive powers.

All the forces, whether attractive or not, that urge the particles of the fluid body H K I, are supposed to be included in the expressions X, Y, Z; and it has been shown that the gravity arising from these forces produces, by its action upon the stratum of which the thickness is K S, equal pressures

upon every point of the surface HKI. The whole mass of fluid, NOL, will therefore be in equilibrio, if it be urged by no other forces. But the attraction of the stratum upon all the matter within it, is a new force brought into play, the efforts of which must be balanced, otherwise the equilibrium could not subsist. Now this new force is distinct from the pressure caused by the gravity, and can never be included in it. Two separate principles must therefore be employed to ensure the equilibrium of the fluid body HKI, when acted upon by the two independent forces. But a fluid body cannot be in equilibrio by the action of external forces upon it, except in one of these two ways: either there must be an equable pressure upon the outer surface; or, all the forces that act upon every separate particle must destroy one another. We are therefore necessarily led to suppose, that the added stratum must possess such a figure as to attract every particle in the inside with equal force in all opposite directions. By the help of this principle, and by no other means, the fluid body HKI, will still continue to be in equilibrio when subjected to the additional pressure, and to the new attractive force.

When more strata are added, they must separately possess the property of attracting every particle in the inside with equal force in opposite directions; by which supposition, we are brought at every step to the same circumstances, as in the case when there is no attraction between the particles. The whole fluid mass being ultimately divided into level strata, the property common to each must belong to the aggregate of any number of them.

On the whole, it is not sufficient for the equilibrium of a

homogeneous fluid body, the particles of which attract one another, that the resultant of the forces X, Y, Z, be directed inward, and that $X\,dx + Y\,dy + Z\,dz$ be an exact differential: to these conditions it is necessary to add that, every particle placed within a stratum bounded by two level surfaces, be in equilibrio by the attraction of the stratum.

The conclusion we have arrived at does not coincide with the usual determination of the equilibrium of a fluid mass, in which no distinction is made between the two cases when the particles attract one another, and when they possess no such powers. The difference arises from this, that no notice is commonly taken of the attraction which the thin level stratum exerts upon the fluid body to which it is added. Every difficulty respecting this point will be removed, if it be impressed on the mind that the gravity at any level surface, and the pressure caused by it, are forces distinct from, and independent of, the attraction of the exterior matter. In estimating the pressure, the exterior fluid is unavoidably regarded merely as inert matter subjected to external force; and when there are active powers inherent in it, the effect of these must be separately investigated. It is said that nothing more is requisite to the equilibrium of a homogeneous fluid, than that the pressure be equable over all the outer surface. For, it is argued, since there is no distinction of density in the interior, it is always possible to trace curves that shall cut at right angles the resultants of all the forces urging the particles; which curves will therefore be level surfaces. But the defectiveness of this reasoning will appear if it be observed that, as every particle of the fluid is attracted by the whole mass, the curve surfaces traced in the manner

described, will be entirely dependent upon the outer surface. If the uppermost stratum, or any number of the uppermost strata be taken away, a part of the attractive force acting upon every particle will be destroyed, and the curve surfaces will no longer be perpendicular to the resultants of the remaining forces urging the particles. Suppose that the strata are taken away successively; then, the figure necessary to the equilibrium of the remaining fluid will change as each stratum is abstracted; which is contrary to the just principles of the equilibrium of a fluid mass. The level surfaces of a homogeneous fluid mass in equilibrio, are determined without ambiguity by varying the arbitrary constant of the general equation. And as there is no doubt that the figure of the outer surface has no relation to any matter placed without it; so any level surface, which is defined by a perfectly similar equation, must be independent of all the exterior matter. Farther, the gravitation acting at any point of the outer surface is a function of the co-ordinates of that point, and has no dependence upon any exterior matter; and, the like force at any level surface being the same function of the co-ordinates of that surface, it must be equally independent of the exterior matter. And although it be admitted that every level surface must be perpendicular to the resultant of all the forces urging the particles, yet it does not follow that no modification of the forces is necessary to the equilibrium. In reality, the foregoing observations prove that, if we reason consistently from what is allowed in the usual determination of the equilibrium of a fluid mass, we shall be led to the same conclusion at which we have already arrived; namely,

that the forces acting at any point in the interior, must be so modified by the figure of the fluid, as to render every level surface, and the gravity at every point of it, independent of the exterior matter.

We may cite as examples of the two different cases of the equilibrium of a homogeneous fluid, the hypothesis of Huyghens respecting the figure of the earth, which falls under the first case; and the Newtonian theory on the same subject, which belongs to the second. Huyghen's supposed an attractive force residing in the earth's centre, and acting with the same intensity at all distances. Therefore, in the case of a revolving mass, every particle is urged by a constant force directed to the centre, and by a centrifugal force proportional to the distance from the axis of rotation. As there is no attraction between particle and particle, a level stratum will act by pressure only upon the fluid below it; and the only condition requisite to the equilibrium, is an equable pressure over all the outer surface. But, according to Newton, every particle attracts every other particle; and a level stratum will act upon the fluid below it, both by the pressure of gravitation and by its own attractive force. In this theory, therefore, the adjustment of the equilibrium requires the joint application of both the principles of the second case.

The method of investigation followed in what goes before, is similar to a process of reasoning in Clairaut's theory of the figure of the earth; and it is certainly surprising that the difference of the two cases was not remarked by that acute geometer. Other authors have very generally adopted a more simple procedure introduced by Euler. It will be

worth while to set before the reader very briefly the steps of EULER's investigation, for the purpose of pointing out the omission with which it is chargeable.

Suppose that the fluid mass in equilibrio is divided into indefinitely small rectangular parallelopipeds by planes parallel to those of the co-ordinates; and let x, y, z, be the co-ordinates of one angle of a parallelopiped which has dx, dy, dz, for its sides, and which we may conceive to be so placed, that $x + dx$, $y + dy$, $z + dz$, are the co-ordinates of the opposite angle. The forces that act upon the parallelopiped are; the pressure of the adjacent fluid upon its six faces; and the accelerating forces X, Y, Z, urging every particle in directions parallel to x, y, z, and tending to increase these lines. The pressure at any point of the fluid must depend upon the situation of that point, or it must be some function φ of the co-ordinates x, y, z: and, according to the principles of the differential calculus, φ will retain the same value over all the three faces of the parallelopiped that comprehend any one of the solid angles. Now, y and z remaining constant, if we substitute $x + dx$ in place of x, φ will be changed into $\varphi + \frac{d\varphi}{dx} \cdot dx$; and the two quantities φ and $\varphi + \frac{d\varphi}{dx} dx$, will represent the intensities of pressure upon the opposite rectangles comprehended by dy and dz: the forces compressing the parallelopiped are therefore $\varphi \times dy\, dz$, and $(\varphi + \frac{d\varphi}{dx} dx)$ $dy\, dz$; and the difference, or $\frac{d\varphi}{dx} dx\, dy\, dz$ is the force causing the parallelopiped to move in a direction tending to diminish x. In like manner, the pressures on the other sides produce the forces $\frac{d\varphi}{dy} dx\, dy\, dz$ and $\frac{d\varphi}{dz} dx\, dy\, dz$, causing the parallelo-

piped to move in directions tending to diminish y and z. Again, the accelerating forces X, Y, Z, acting on every parallelopiped produce the motive forces $X\,dx\,dy\,dz$, $Y dx dy dz$, $Z dx dy dz$, tending to increase the lines x, y, z. But the equilibrium of the parallelopiped requires the equality of the opposite forces: wherefore,

$$\frac{d\varphi}{dx} = X, \frac{d\varphi}{dy} = Y, \frac{d\varphi}{dz} = Z.$$

Hence, we get,

$$d\varphi = X\,dx + Y\,dy + Z\,dz.$$

Wherefore if we trace a stratum of the fluid so that φ shall every where have the same value, the figure of the stratum will be defined by the equation

$$X\,dx + Y\,dp + Z\,dz = o;$$

which likewise shows that the resultant of the accelerating forces is perpendicular to the stratum.

In what has been said, the equilibrium of every parallelopiped is established with respect to all the outward forces extrinsic to its own matter. If the question relate to no other forces, the whole fluid mass, and all the level strata of which it consists, will be in equilibrio, and the problem is solved. But when the particles of the fluid attract one another, there are forces not yet taken into account, inherent in every parallelopiped, by means of which it will act upon all the exterior matter, and the efforts of which must be balanced, otherwise the equilibrium could not subsist. Now, if we suppose, as before, that all the level strata are possessed of such a figure as to act upon particles in the inside with equal forces in opposite directions, it is evident, that every parallelopiped will be in equilibrio by its action upon all the matter on the

outside of the stratum. With regard to the matter in the inside, a parallelopiped will act upon it effectively; but, the united attraction of all the parallelopipeds in the same stratum upon every interior particle being equal in opposite directions, it will not disturb the equilibrium of the fluid below the stratum. Therefore, when we take into account all the forces that act upon the parallelopipeds; both those urging them externally, and those inherent in their own matter; it is evident, that all the molecules in the same level stratum will be in equilibrio with respect to the matter above them, and that they will press equably upon the fluid body below them, by the action of the gravity alone. The fluid mass will therefore be in equilibrio with respect to all the forces in action. Thus, in every view of the problem, it appears that, when nothing essential is omitted, the particular conformation of the level strata which annihilates their action upon particles in the inside, is just as necessary to the equilibrium of the fluid mass, as the equality between the pressure and the effect of the accelerating forces.

There is another way of arriving at the same conclusion, which, in reality, first led to the suspicion of some defect lurking in the usual determination of the equilibrium of a fluid mass. This new view of the subject, which applies only to the law of attraction that takes place in nature, is contained in the two following propositions.

Proposition I.

If a homogeneous fluid body revolving about an axis, be in equilibrio by the attraction of its particles in the inverse proportion of the square of the distance; any other mass of

the same fluid having a similar figure, and revolving with the same rotatory velocity about an axis similarly placed, will likewise be in equilibrio, supposing that its particles attract one another by the same law.

Suppose that a homogeneous fluid body revolves about the axis A B, and is in equilibrio by the attraction of its particles and the centrifugal force; and let another mass of the same fluid, similar in its figure to the first body, revolve, in the same time, about the axis ab, similarly situated to A B: this latter body will also be in equilibrio.

Conceive that the body in equilibrio is divided into an indefinitely great number of thin level strata; and let the other body be divided into the same number of strata by surfaces similar, and similarly situated to the level surfaces of the first body. Take any point H in a level surface of the body in equilibrio; and in the corresponding surface of the other body, let the point h be similarly situated to H. Farther, suppose the two bodies are similarly divided into the same indefinitely great number of molecules, of which dm and dm' are any two situated alike, and therefore having their volumes and quantities of matter proportional to the volumes and quantities of matter of the two whole bodies: and let f and f' denote the respective distances of the points H and h from dm and dm', and r and r', their respective distances from the axes A B and ab.

The forces with which the molecules dm and dm' attract the points H and h (which must be considered as two equal particles of matter) are proportional to $\frac{dm}{f^2}$ and $\frac{dm'}{f'^2}$: and, in these fractions, the numerators being proportional to the

cubes, and the denominators to the squares, of any two homologous lines of the respective bodies, the attractive forces will be simply proportional to any two such lines. The lines f and f', in the directions of which the forces act, are likewise similarly inclined to the surfaces passing through the two points H and h. It follows, therefore, that the forces with which the similar molecules into which the two bodies are divided, attract the points H and h, are constantly in the same proportion to one another, and act in directions that make like angles with the surfaces passing through the same points. Farther, since the velocity of rotation is the same in the two bodies, the centrifugal forces urging the points H and h will be proportional to the respective distances from the axes A B and $a\,b$; that is, to r and r'; or to any homologous lines of the respective bodies; and the same forces, having their directions in the prolongations of r and r', make like angles with the surfaces passing through H and h. Wherefore all the accelerating forces urging the points H and h, are respectively in the same proportion to one another, and have like inclinations to the surfaces passing through the same points. Consequently, the resultants of the same forces will follow the like proportion, namely, that of any homologous lines; and they will likewise be similarly inclined to the two surfaces. But the resultant of the accelerating forces acting at H, is perpendicular to the level surface passing through that point; wherefore, the resultant of the accelerating forces acting at h, is likewise perpendicular to the surface in which that point is placed, and has to the other resultant the same proportion of any two homologous lines of the respective bodies. And thus, as in the body in equilibrio, the gravity, or the resultant of the accelerating forces

is every where perpendicular to the level surfaces; so in the other body, the like force is every where perpendicular to the surfaces similarly situated.

Take K and k any other two points similarly situated in the same surfaces that contain H and h: and suppose that HM, KN, are the thicknesses of the level stratum, in the upper surface of which H and K are placed; and, in like manner, let hm, kn, be the thicknesses of the like stratum in the other body. Farther, put G, G′ for the resultants of the accelerating forces, or the gravitations, at H and K; and g, g' for the like forces at h, k. Because H and h are points similarly situated, the forces G, g are proportional to any homologous lines of the respective bodies. The same thing is true of the forces G′, g'. Wherefore,

$$G : G' :: g : g'.$$

But the line HM is homologous to hm, and KN, to kn: wherefore,

$$HM : KN :: hm : kn.$$

Consequently,

$$G \times HM : G' \times KN :: g \times hm : g' \times km.$$

But the proportion of G × HM to G′ × KN is equal to that of the pressures of the stratum upon the fluid below it at the points H and K: for the quantities of matter in the stratum are proportional to the thicknesses HM and KN; and the pressures are proportional to the gravitations multiplied by the quantities of matter. In like manner $g \times hm$ and $g' \times kn$, are proportional to the pressures of the stratum upon the fluid below it at the points h and k. Wherefore the pressures at H and K are proportional to the pressures at h and k. And, in general, taking any points similarly placed in the two corresponding surfaces, the pressures of the stratum upon the

fluid below it in one body are in the same proportion to one another, as the pressures of the stratum upon the fluid below it in the other body. But in the body in equilibrio, the pressures at all points are equal; wherefore, in the other body, a stratum likewise presses equably upon the fluid below it. And what is true of each individual stratum, must be true of the accumulated pressure of any number of superincumbent strata.

Thus, in the two bodies, every thing is similar. The forces which urge the particles of one, are, in the case of the other, all increased, or all diminished, in the same proportion, while they act in like directions. If, in the one, the gravity be every where perpendicular to the level surfaces; the like force is perpendicular to the surfaces similarly traced in the other: and if, in the first, all the level strata press equably upon the fluid below them; the same thing is true of the strata into which the second is divided. Wherefore, the equilibrium of one body is a necessary consequence of the equilibrium of the other.

Proposition II.

If a homogeneous fluid mass revolve about an axis, and be in equilibrio by the attraction of its particles in the inverse proportion of the square of the distance; all the level surfaces will be similar to the outer one: and any stratum of the fluid contained between two level surfaces will attract particles in the inside with equal force in opposite directions.

Suppose that the homogeneous fluid body R S T, revolving about the axis A B, is in equilibrio by the centrifugal force, and the attraction of its particles in the inverse proportion of

the square of the distance. The axis of rotation A B, will pass through G, the centre of gravity of the fluid mass. In the interior of the revolving body, trace, round the point G, any surface H I K, similar and similarly situated to the outer surface. Then the whole fluid body R S T, and the part of it bounded by the surface H I K, are similar to one another in their figure; and they revolve about the common axis A B, which cuts them both similarly: wherefore, because the first body is in equilibrio, the latter body will also be in equilibrio, supposing that it revolves by itself, the exterior matter being taken away, or annihilated.*

And, because the body H I K is in equilibrio, when it revolves by itself, the resultant of the forces acting at its surface (namely, the attraction of its own particles and the centrifugal force) will, at every point, be perpendicular to that surface.

Suppose now that all the fluid exterior to the surface H I K is divided into very thin strata by the surfaces O P Q, L M N, similar and similarly situated to the outer surface R S T. Then, understanding by the gravitation at any of the surfaces O P Q, L M N, &c. the resultant of the centrifugal force and the attraction of the fluid matter within that surface, it has been proved that these gravitations are perpendicular to the respective surfaces. Wherefore the uppermost stratum will be pressed perpendicularly upon the surface O P Q by the gravitation at that surface. For the same reason the next stratum will be pressed perpendicularly upon the surface L M N. And, in like manner, the successive strata will be pressed perpendicularly, each upon the surface on which

* Proposition I.

it lies, by the respective gravitations. If we conceive a curve line GHLOR, extending from the common centre G so as to cut all the similar surfaces at right angles; that curve line will mark the directions both of the gravitation and the pressure of the fluid in the interior of the body RST. Wherefore the several surfaces OPQ, LMN, &c. are no other than the level surfaces of the body RST in equilibrio; and each of these surfaces will be pressed by the superincumbent fluid with the same intensity over its whole extent.

But at the same time that the uppermost stratum presses upon the fluid below it, by the gravitation at the surface OPQ, it likewise attracts every particle of matter within the same surface. And, in like manner, every successive stratum both presses on the surface on which it lies by the gravitation at that surface, and attracts all the particles within it. Wherefore the body HIK is not only pressed by the superincumbent fluid, but every particle of it, is likewise attracted by all the exterior matter. These forces are independent on one another. Although the body HIK be in equilibrio with respect to the pressure it sustains, it does not follow that it will likewise be in equilibrio with respect to the attraction which the exterior matter exerts upon it. In order that this latter equilibrium take place, it is necessary that every stratum of the exterior matter be possessed of such a figure as to attract all particles in the inside with equal force in opposite directions.

We have now proved that, if the fluid mass RST be in equilibrio, the interior body HIK will likewise be in equilibrio when it revolves by itself, the exterior matter being taken away, or annihilated; which cannot be the case, unless

the same body H I K be in equilibrio with respect to the pressure and attraction which the exterior matter exerts upon it. It has likewise been proved that all the surfaces O P Q, L M N, &c. similar to the outer surface R S T, are level surfaces; and this ensures the equilibrium of the interior body H I K with respect to the pressure it sustains. Its equilibrium with respect to the attraction of the exterior matter, requires farther, that all the strata between the surfaces R S T, O P Q, L M N, &c. attract every particle within them equally in opposite directions. We are therefore to conclude that the homogeneous fluid body R S T, which revolves about the axis A B, and the particles of which attract one another in the inverse proportion of the square of the distance, cannot be in equilibrio, unless both these conditions be fulfilled; 1st. The level surfaces must be all similar to one another; 2dly. Every stratum contained between two level surfaces must attract particles in the inside with equal force in opposite directions.

In the Proposition that has just been proved, the similarity of the level surfaces is an accidental property connected with the supposed law of attraction. In the general hypothesis of an attractive power between the particles, the conditions of equilibrium are no more than these: 1st. The resultant of the accelerating forces acting at every point of the outer surface must be directed at right angles towards that surface; 2dly. All the level strata must possess such a figure as to attract particles in the inside with equal force in opposite directions. It may not be altogether superfluous to prove, by a synthetic demonstration, that these conditions are sufficient for the equilibrium. This is done in the following Proposition.

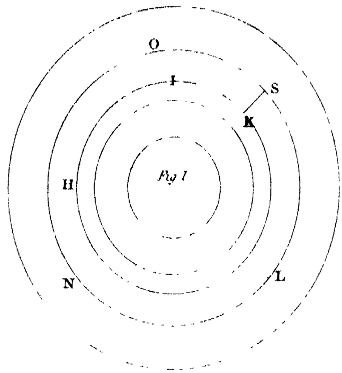

Fig 1

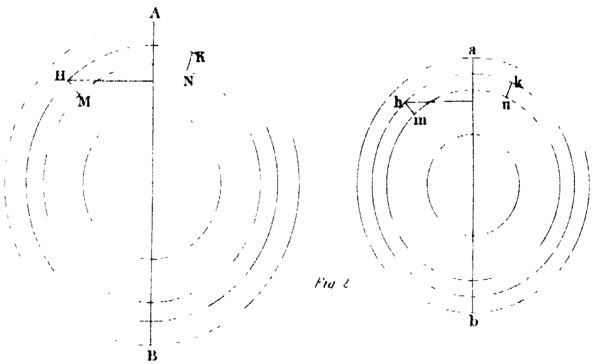

Fig 2

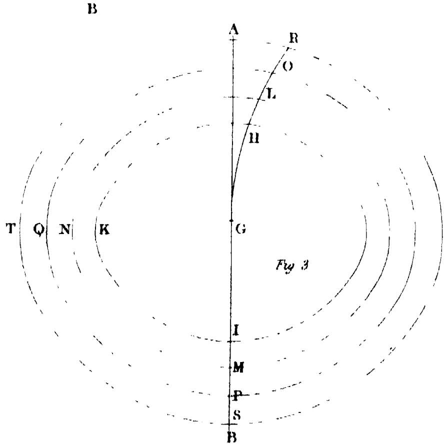

Fig 3

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Proposition III.

If a homogeneous fluid mass fulfil the two above-mentioned conditions, it is in equilibrio.

Let the equation of the outer surface of the fluid mass, be

$$\phi = C;$$

ϕ representing a function of three rectangular co-ordinates, x, y, z. Then the accelerating forces parallel to the axes of x, y, z, will be respectively equal to $\frac{d\phi}{dx}, \frac{d\phi}{dy}, \frac{d\phi}{dz}$; and the condition that the resultant of these forces is perpendicular to the surface of the fluid will be expressed by the differential equation,

$$\frac{d\phi}{dx} dx + \frac{d\phi}{dy} dy + \frac{d\phi}{dz} dz = o.$$

We shall suppose that the whole fluid mass is divided into thin strata by level surfaces, which are determined by making the constant quantity C vary by insensible degrees in the equation of the outer surface. Farther, let the thickness of the uppermost stratum be denoted by the line k, drawn perpendicular to the outer surface from a point of which the co-ordinates are x, y, z; and let $x - \delta x$, $y - \delta y$, $z - \delta z$, be the co-ordinates of the other extremity of k in the under surface of the stratum. The equation of this surface will be found by substituting $x - \delta x$, $y - \delta y$, $z - \delta z$, in place of x, y, z, in the function ϕ, and by changing C into $C - \delta C$; it will therefore be,

$$\phi - \frac{d\phi}{dx} \delta x - \frac{d\phi}{dy} \delta y - \frac{d\phi}{dz} \delta z = C - \delta C:$$

and by subtracting this from the equation of the outer surface, we get

$$\frac{d\phi}{dx}\delta x + \frac{d\phi}{dy}\delta y + \frac{d\phi}{dz}\delta z = \delta C.$$

Again; if we put,

$$p = \sqrt{\left(\frac{d\phi}{dx}\right)^2 + \left(\frac{d\phi}{dy}\right)^2 + \left(\frac{d\phi}{dz}\right)^2};$$

then, because the line k is perpendicular to the outer surface, it follows, from the known properties of curve surfaces, that the cosines of the angles which k makes with the co-ordinates, are respectively equal to,

$$\frac{1}{p} \times \frac{d\phi}{dx}; \ \frac{1}{p} \times \frac{d\phi}{dy}; \ \frac{1}{p} \times \frac{d\phi}{dz}:$$

and hence,

$$\delta x = \frac{k}{p} \times \frac{d\phi}{dx}; \ \delta y = \frac{k}{p} \times \frac{d\phi}{dy}; \ \delta z = \frac{k}{p} \times \frac{d\phi}{dz}:$$

and by substituting these values in the preceding formula, we obtain,

$$\frac{k}{p} \times \left\{\left(\frac{d\phi}{dx}\right)^2 + \left(\frac{d\phi}{dy}\right)^2 + \left(\frac{d\phi}{dz}\right)^2\right\} = \frac{k}{p} \times p^2 = k \times p = \delta C.$$

Now p is the resultant of the accelerating forces at the surface: and the line k, or the thickness, is proportional to the quantity of matter in the stratum at the same point; wherefore $k \times p$ is the pressure; and the formula,

$$k \times p = \delta C,$$

shows that the uppermost stratum presses upon the fluid below it equally at all points.

As the attraction of the whole fluid mass is one of the component forces of the gravitation p, the attraction of the stratum must enter as a part of the same force. But it is evidently only an infinitely small part of it; and consequently produces only an infinitely small part of the pressure $p \times k$. We may therefore consider the gravitation at the

outer surface, and the pressure of the uppermost stratum upon the fluid below it, as both independent of the attractive force of the matter of the stratum. But the attraction of the same matter upon all the particles within the stratum is a force of the same order with the pressure $p \times k$, and comparable with it, and which must not be neglected. Thus it appears, that the uppermost stratum acts upon the fluid below it both by pressure and by attraction; and, as all the level strata are derived from one another by the same law, it follows, that every stratum in the interior likewise acts upon the fluid below it both by pressure and by attraction.

Now it has already been shown, that the pressure of the uppermost stratum is the same over all the surface of the fluid below it; and the same thing, it is manifest, is equally true of any level stratum. Wherefore, since the strata press equably upon one another, any fluid body in the interior, bounded by a level surface, will be in equilibrio with respect to the pressure it sustains from all the superincumbent strata. But, according to the second condition in the hypothesis of the proposition, the same body will also be in equilibrio with respect to the attraction of all the exterior strata. Thus, every interior fluid body bounded by a level surface, is in equilibrio with respect to all the forces which the exterior matter exerts upon it. And, as this is true independently of the dimensions of the interior body, we may suppose that it is ultimately reduced to a quantity infinitely small, which exerts no force, and is in equilibrio by the external forces acting upon it. Then the whole fluid mass will be resolved into level strata, that are in equilibrio with respect both to the pressures and to the attractive forces, which they exert

upon one another. We are therefore to conclude, that the two conditions of the proposition are sufficient to ensure the equilibrium of the fluid mass, and that both are necessary to produce the effect.

5. Having established the physical properties of a homogeneous fluid mass in equilibrio, the investigation of its figure, which is now brought within the power of analysis, is not attended with much difficulty.

If a homogeneous body of fluid revolve about an axis, and be in equilibrio by the centrifugal force and the attraction of its particles, the axis of rotation will pass through the centre of gravity. This point is to be supposed at rest; since it is not the effect of any external forces that we have to consider, but merely the mutual action of the particles upon one another. If one of the planes of the co-ordinates pass through the centre of gravity at right angles to the axis of rotation, the other two will intersect one another in the same axis. Let a, b, c, denote the co-ordinates of an attracted point (which must be considered as some small particle of the fluid containing a given quantity of matter) placed any where in the mass, a being parallel to the axis of rotation; and put $r = \sqrt{a^2 + b^2 + c^2}$. Suppose also that $V(r)$ denotes the sum of all the molecules of the body divided by their respective distances from the attracted point: then $\frac{d.V(r)}{da}$, $\frac{d.V(r)}{db}$, $\frac{d.V(r)}{dc}$, will be the accumulated attractive forces exerted upon the attracted point by the whole mass, in the directions of a, b, c, and tending to increase these lines. Again; let ω be the centrifugal force at the distance from the axis of rotation equal to unit; then, the distance of the attracted point from the same axis being equal to $\sqrt{b^2 + c^2}$, the centrifugal force

urging it from the axis, will be $\omega \times \sqrt{b^2 + c^2}$; and the effect of the same force to lengthen b and c, will be equal to $\omega \times \sqrt{b^2 + c^2} \times \frac{b}{\sqrt{b^2 + c^2}}$ and $\omega \times \sqrt{b^2 + c^2} \times \frac{c}{\sqrt{b^2 + c^2}}$, or to $\omega\, b$ and $\omega\, c$. Hence, the forces acting upon the attracted point, and tending to increase a, b, c, are respectively,

$$\frac{d \,.\, V(r)}{d a},$$
$$\frac{d \,.\, V(r)}{d b} + \omega \,.\, b,$$
$$\frac{d \,.\, V(r)}{d b} + \omega \,.\, c.$$

Now, the resultant of these forces must be perpendicular to the level surface of which a, b, c are the co-ordinates; which condition is expressed by this differential equation, viz.

$$\frac{d \,.\, V(r)}{d a} d a + \frac{d \,.\, V(r)}{d b} d b + \frac{d \,.\, V(r)}{d c} d c + \omega\,(b\, d b + c\, d c) = o:$$

and the integral of this, viz.

$$V(r) + \frac{\omega}{2}(b^2 + c^2) = C,$$

is the general equation of all the level surfaces. Let μ denote the cosine of the angle which the line r makes with the axis of rotation; and the foregoing equation will become,

$$V(r) + \frac{\omega}{2} \,.\, r^2 (1 - \mu^2) = C.$$

And if we put R for the radius of the outer surface of the fluid body, we shall have, for the equation of that surface,

$$V(R) + R^2 \times \frac{\omega}{2} (1 - \mu^2) = C, \ldots (A)$$

which is one condition of the equilibrium of the fluid mass.

The equation just found is an essential condition of the equilibrium, although it is not the only one. As it merely expresses that the resultant of all the accelerating forces is perpendicular to the fluid's surface, it is not confined to a homogeneous body, nor to one entirely fluid, but is true in

every case when a fluid in equilibrio covers, either entirely or partially, the surface of any body, however variable in structure or density. Now, from the equation, we get,

$$\frac{1}{R^2} = \frac{\frac{V(R)}{R^2} + \frac{\omega}{2}(1-\mu^2)}{C};$$

and, as it has been proved in §. 1, that $\frac{V(R)}{R^2}$ is always a function of three rectangular co-ordinates of a point in the surface of a sphere, it follows that $\frac{1}{R^2}$, and consequently R, must be like functions. This inference, being founded on considerations of the most general nature, cannot but include every case of a fluid in equilibrio, placed upon the surface of a revolving body.

Again, suppose that $R_{,}$ is the radius of any level surface which contains the attracted point within it; and let $V_{,}(r)$ denote the sum of all the molecules of the fluid within the level surface, divided by their respective distances from the attracted point: then,

$$V(r) - V_{,}(r)$$

will be the sum of the molecules in the stratum of fluid contained between the outer surface and the level surface, divided by their respective distances from the attracted point; and the attractive forces of the stratum upon the attracted point in the directions of a, b, c, will be respectively,

$$\frac{d \,.\, \left\{ V(r) - V_{,}(r) \right\}}{da},$$

$$\frac{d \,.\, \left\{ V(r) - V_{,}(r) \right\}}{db},$$

$$\frac{d \,.\, \left\{ V(r) - V_{,}(r) \right\}}{dc}.$$

Now, in the case of a homogeneous mass of fluid in equilibrio, these forces must be evanescent; and that too, for every point within the stratum, which requires that

$$V(r) - V_{,}(r),$$

shall be a constant quantity independent of a, b, c. And this is the remaining condition necessary to the equilibrium.

Now, if we put,

$$Q = r^2 \int - \frac{dr}{r^3} \iint \frac{R'^3\, d\mu'\, d\varpi'}{\sqrt{r^2 - 2rR'\gamma + R'^2}}$$

$$Q_{,} = r^2 \int - \frac{dr}{r^3} \iint \frac{R'^3_{,}\, d\mu'\, d\varpi'}{\sqrt{r^2 - 2rR'_{,}\gamma + R'^2_{,}}};$$

we shall get,

$$V(r) = Q + Kr^2$$

$$V_{,}(r) = Q_{,} + K_{,}r^2 :*$$

consequently,

$$V(r) - V_{,}(r) = Q - Q_{,} + r^2(K - K_{,}).$$

A very little attention will show that Q and $Q_{,}$, contain no terms multiplied by r^2. For if we expand Q into a series of the ascending powers of r, the term containing r^2, will be

$$r^2 \times \iint C^{(2)}\, d\mu'\, d\varpi',$$

which, by the nature of the function $C^{(2)}$, is equal to zero. Wherefore the foregoing expression cannot be independent of a, b, c, unless

$$K - K_{,} = o; \text{ and } K = K_{,}.$$

But it has already been shown that the equality of K to $K_{,}$ requires that the radii R' and $R'_{,}$, which make the same angles with the axes of the co-ordinates, and consequently are in the same straight line, be constantly in the same proportion to one another:* and hence we obtain this property of a homo-

* Section 3.

geneous fluid mass in equilibrio, namely, that all the level surfaces are similar to the outer one.

Again, since R and R', are always in the same proportion to one another, $Q - Q_{,}$ will be independent of a, b, c, if we make r disappear in Q. Now, by expanding the expression of Q, and equating the co-efficients of the several powers of r to zero, we get,

$$Q = \int\int R'^2 d\mu' d\varpi$$

$$o = \int\int R' . C^{(1)} d\mu' d\varpi'$$

$$o = \int\int \frac{C^{(3)} d\mu' d\varpi'}{R'}$$

$$o = \int\int \frac{C^{(4)} d\mu' d\varpi'}{R'}$$

and generally,

$$o = \int\int \frac{C^{(i)} d\mu' d\varpi'}{R'^{i-2}}$$

In the first place, all these equations are satisfied if we suppose R' constant; that is, if the figure of the fluid be a sphere. But the supposition of a sphere is inconsistent with the equation (A), unless ω be evanescent. Wherefore, a homogeneous fluid body of a spherical figure cannot be in equilibrio by the attraction of its particles, unless it have no rotatory motion.

Again, it follows from what has been shown, that R' is a function of μ', $\sqrt{1-\mu'^2}$. Sin ϖ, $\sqrt{1-\mu'^2}$. Cos ϖ'; μ' being the cosine of an arc θ' reckoned from the pole of a great circle on the sphere, and ϖ' the angle between θ' and a given great circle passing through the same pole. Now if we suppose that R' is an even function, or that it contains only the squares and the combinations of the squares of μ',, $\sqrt{1-\mu'^2}$. Sin ϖ', $\sqrt{1-\mu'^2}$. Cos ϖ'; the values of R', which are always

positive, will be the same in quantity, at points diametrically opposite on the sphere, at which points μ', Sin. ϖ' and Cos. ϖ', are different only in their signs. And because,

$$\gamma = \mu\,\mu' + \sqrt{1-\mu^2} \,.\, \sqrt{1-\mu'^2}\,\text{Cos.}\,(\varpi - \varpi'),$$

it is obvious that γ will likewise have the same value and different signs at any two points diametrically opposite on the sphere; and the same property will belong to every function of γ that contains only the odd powers. Now we have

$$R' \,.\, C^{(1)}\, d\mu'\, d\varpi' = -R' \,.\, C^{(1)}\, d\theta' \,\text{Sin}\, \theta'\, d\varpi';$$

and, as θ' increases from o to π, and ϖ' from o to 2π, it is obvious that the fluxions will be the same in quantity, but will have different signs, at any two points diametrically opposite on the sphere; because the sign of $C^{(1)}$, which is an odd function of γ, alone changes. Wherefore the integral will decrease just as much in one hemisphere as it increases in the other; and being extended to the whole sphere, it will be equal to zero. In the same manner it is proved that

$$\iint \frac{C^{(i)}\, d\mu'\, d\varpi'}{R'^{\,i-2}} = o,$$

whenever i is an odd number. Thus all the equations we are considering, in which i is an odd number, are satisfied by the supposition that R' is an even function of μ',, $\sqrt{1-\mu'^2}$. Sin ϖ', $\sqrt{1-\mu'^2}$. Cos ϖ'.

It remains to consider the cases when i is an even number, viz.

$$\iint \frac{C^{(4)}\, d\mu'\, d\varpi'}{R'^2} = o$$

$$\iint \frac{C^{(6)}\, d\mu'\, d\varpi'}{R'^4} = o$$

$$+\ \&c.$$

For this purpose the following theorem is premised, viz.

Theorem. If m, m', m'' denote any positive integer numbers, such that $m + m' + m''$ is less than i; then

$\iint \mu'^{m} . (\sqrt{1-\mu'^2} . \text{Sin.}\,\varpi')^{m'} . (\sqrt{1-\mu'^2} . \text{Cos.}\,\varpi')^{m''} . C^{(i)} d\mu' d\varpi' = 0$,

the integral being extended from $\mu' = 1$ to $\mu' = -1$, and from $\varpi' = 0$ to $\varpi' = 2\pi$.

As expressions of this kind have been very amply discussed, and as the theorem follows very readily from the properties generally known, I shall not stop to give the demonstration.

It follows from the theorem that the equation,

$$\iint \frac{C(4) d\mu' d\varpi'}{R'^2} = 0,$$

cannot be true, if $\frac{1}{R'^2}$ contain any even power of the quantities μ', $\sqrt{1-\mu'^2} . \text{Sin.}\,\varpi'$, $\sqrt{1-\mu'^2} . \text{Cos.}\,\varpi'$, above the square; or if it contain any product of two or more of the squares of the same quantities. Wherefore the most general value of $\frac{1}{R'^2}$, consistent with the above equation, is

$$\frac{1}{R'^2} = A\mu'^2 + B(1-\mu'^2)\,\text{Sin.}^2\,\varpi' + C(1-\mu'^2)\,\text{Cos.}^2\,\varpi'.$$

It may be observed, that this expression would not be more general by adding an absolute quantity, as D: for, since

$$\mu'^2 + (1-\mu'^2)\,\text{Sin.}^2\,\varpi' + (1-\mu'^2)\,\text{Cos.}^2\,\varpi' = 1,$$

such a quantity would blend itself with the other terms. But the same value of $\frac{1}{R'^2}$ will likewise satisfy all the equations,

$$\iint \frac{C^{(i)} d\mu' d\varpi'}{R'^{i-2}} = 0,$$

in which i is an even number. For, because

$$\frac{1}{R'^{i-2}} = (A\mu'^2 + B(1-\mu'^2)\,\text{Sin.}^2\,\varpi' + C(1-\mu'^2)\,\text{Cos.}^2\,\varpi')^{\frac{i-2}{2}},$$

it follows that the expansion of $\frac{1}{R^{i-2}}$ will produce no quantities in the integral except such as are evanescent by the theorem.

The most general value of $\frac{1}{R'^2}$, consistent with one of the conditions of the equilibrium, has now been found. If we write $\frac{1}{k^2}$, $\frac{1}{k'^2}$, $\frac{1}{k''^2}$, for A, B, C, we shall get,

$$\frac{1}{R'^2} = \frac{\mu'^2}{k^2} + \frac{(1-\mu'^2)\,\text{Sin.}^2\,\varpi'}{k'^2} + \frac{(1-\mu'^2)\,\text{Cos.}^2\,\varpi'}{k''^2};$$

an equation which belongs to an ellipsoid of which k, k', k'', are the three semi-axes. It is therefore proved that a homogeneous fluid mass cannot be in equilibrio by the attraction of its particles, and a centrifugal force of rotation, unless its figure be included in the ellipsoids. But it is still to be shown that the same figure is consistent with the other condition of the equilibrium. For the sake of abridging, put

$$S = \mu'^2 + \frac{k^2}{k'^2}(1-\mu'^2)\,\text{Sin.}^2\,\varpi' + \frac{k^2}{k''^2}(1-\mu'^2)\,\text{Cos.}^2\,\varpi';$$

then, $$R'^2 = \frac{k^2}{S}; \text{ and } R' = \frac{k}{\sqrt{S}}.$$

The value of Q being reduced to the first term of its expansion, we have,

$$Q = \int\int R'^2\, d\mu'\, d\varpi' = k^2 . \int\int \frac{d\mu'\, d\varpi'}{S}:$$

and hence,

$$V(R) = Q + K . R^2 = k^2 . \int\int \frac{d\mu'\, d\varpi'}{S} + KR^2.$$

Let this value be substituted in the equation (A), and we shall obtain,

$$R^2 = \frac{k^2 \int\int \frac{d\mu'\, d\varpi'}{S} - C}{K + \frac{\omega}{2}(1-\mu^2)}$$

And the solution of the problem is now reduced to show that this formula is similar to the equation

$$R'^2 = \frac{k^2}{S}.$$

When this is done, the relation between the figure of the fluid mass and the given rotatory velocity will be found by making the two expressions of R and R′ coincide, so that both shall belong to the same surface.

In the first place, the integral in the numerator of the value of R^2 is a function of k, k', k''; and as any value may be assigned to C, the whole numerator may be regarded as an arbitrary quantity. The denominator is therefore all that remains to be considered. Now,

$$K = -\frac{2\pi}{3} + \iint \text{Log. } R' \times C^{(2)} d\mu' d\varpi';$$

and, since $\text{Log. } R' = \text{Log. } k - \frac{1}{2} \text{Log. } S$, we shall get,

$$K = -\frac{2\pi}{3} + \tfrac{1}{2}\iint - \text{Log. } S \times C^{(2)} d\mu' d\varpi',$$

because, $\qquad \text{Log. } k \times \iint C^{(2)} d\mu' d\varpi' = o.$

Again, $\qquad C^{(2)} = \frac{1}{2.4} \cdot \frac{d^2 . (1-\gamma^2)^2}{d\gamma^2} = \frac{3}{2}\gamma^2 - \frac{1}{2};$

and, $\gamma = \mu\mu' + \sqrt{1-\mu^2} \cdot \sqrt{1-\mu'^2} \,.\, \text{Cos. } (\varpi - \varpi')$;

or, if we write m, n, p, for $\mu, \sqrt{1-\mu^2}.\text{ Sin. } \varpi, \sqrt{1-\mu^2}.\text{ Cos. } \varpi$, we shall obtain,

$$\gamma = m\mu + n\sqrt{1-\mu'^2}\,.\,\text{Sin. } \varpi + p\sqrt{1-\mu'^2}\,.\,\text{Cos. } \varpi':$$

and hence,

$$\begin{aligned} K = -\frac{2\pi}{3} &+ \frac{m^2}{2}\iint - \left(\frac{3}{2}\mu'^2 - \frac{1}{2}\right) \text{Log. S} \,.\, d\mu' d\varpi' \\ &+ \frac{n^2}{2}\iint - \left\{\frac{3}{2}(1-\mu'^2)\text{ Sin.}^2\varpi' - \frac{1}{2}\right\} \text{Log. S}.d\mu' d\varpi' \\ &+ \frac{p^2}{2}\iint - \left\{\frac{3}{2}(1-\mu'^2)\text{ Cos.}^2\varpi' - \frac{1}{2}\right\} \text{Log. S}.d\mu' d\varpi' \\ &+ 3\,mn\iint - \mu'\sqrt{1-\mu'^2}.\text{ Sin. } \varpi' \text{ Log. S. } d\mu' d\varpi' \\ &+ 3\,mp\iint - \mu'\sqrt{1-\mu'^2}.\text{ Cos. } \varpi' \text{ Log. S}.d\mu' d\varpi' \\ &+ 3\,np\iint - (1-\mu'^2)\text{ Sin.}\varpi' \text{ Cos.}\varpi' \text{Log. S}.d\mu' d\varpi'. \end{aligned}$$

But, because $\mu' = \text{Cos.}\,\theta'$, the three last integrals will become,

$$\int\int \text{Sin.}^2\,\theta'\ \text{Cos.}\,\theta'\ \text{Sin.}\ \varpi'\ \text{Log.}\ S\ .\ d\,\theta'\,d\,\varpi'$$

$$\int\int \text{Sin.}^2\,\theta\ \ \text{Cos.}\,\theta'\ \text{Cos.}\ \varpi'\ \text{Log.}\ S\ .\ d\,\theta'\,d\,\varpi'$$

$$\int\int \text{Sin.}^3\,\theta'\ \text{Sin.}\ \varpi'\ \text{Cos.}\ \varpi'\ \text{Log.}\ S\ .\ d\,\theta'\,d\,\varpi':$$

and, attending to the expression of S, it will follow that, in the two first integrals, if we suppose ϖ' to remain constant and θ' to vary from o to 180°; the fluxions will be equal, but will have different signs, at equal distances from o and 180°: wherefore the integrals, taken between the prescribed limits, are evanescent. In the third integral, if we suppose θ' to remain constant and ϖ' to vary from o to 360°, the fluxions will be equal, but will have different signs, at equal distances from o and 180° in the first semicircle: and at equal distances from 180° and 360° in the second semicircle: wherefore the whole integral is evanescent. Rejecting therefore the three last terms of the value of K, and representing the three remaining integrals by L, M, N, we shall get,

$$\begin{aligned} K + \frac{\omega}{2}(1-\mu^2) = {} & \left(\frac{L}{2} - \frac{3}{2}\pi\right) . \mu^2 \\ & + \left(\frac{M}{2} - \frac{3}{2}\pi + \frac{\omega}{2}\right) . (1-\mu^2)\ \text{Sin.}^2\,\varpi \\ & + \left(\frac{N}{2} - \frac{3}{2}\pi + \frac{\omega}{2}\right) . (1-\mu^2)\ \text{Cos.}^2\,\varpi. \end{aligned}$$

This is the denominator in the formula for R^2, and it is entirely similar to the expression of S. Wherefore the two values of R^2 and R'^2 are alike in point of form; and the figure of the fluid mass that corresponds to the given rotatory velocity will be determined by making them coincide.

We are now to conclude that a homogeneous fluid mass cannot be in equilibrio by the attraction of its particles and a centrifugal force of rotation, unless it have the figure of an

ellipsoid; and farther, that an ellipsoid may be found that will fulfil all the conditions of the equilibrium, unless there be some cases in which the necessary relations between the figure and the given velocity of rotation, lead to equations that cannot be solved.

6. In order to apply the foregoing solution, it becomes necessary to compute the integrals L, M, N; or, in other words, to find the attractive forces of an ellipsoid upon a point in the surface. If we extend the problem generally to a point within or without the figure, it is attended with some difficulty; and it is usual to deduce the latter case from the former, which is more easily solved. There is however a great analogy between the two cases; or rather the distinction between them may be dispensed with; since the supposition of a point within the figure is equivalent to that of a point in the surface, which is the extreme case of a point without the figure. In this view the problem admits of a general solution deducible, by a short analysis, from the transformations used in this Paper.

Suppose that k, k', k'', represent the semi-axes of an ellipsoid; and let x, y, z, respectively parallel to the axes, denote the three co-ordinates of a point in the surface of the figure. Farther let a, b, c, be the co-ordinates of an attracted point without the figure; and conceive another ellipsoid, the surface of which passes through the attracted point, and which has its principal sections in the same planes with the principal sections of the given ellipsoid, and also the differences of the squares of its semi-axes h, h', h'', equal to the differences of the squares of k, k', k''; that is, $h'^2 - h^2 = k'^2 - k^2$, and $h''^2 - h^2 = k''^2 - k^2$. The equations of the two curve surfaces will thus be,

$$\frac{x^2}{k^2}+\frac{y^2}{k'^2}+\frac{z^2}{k''^2}=1$$

$$\frac{a^2}{h^2}+\frac{b^2}{h'^2}+\frac{c^2}{h''^2}=1,$$

$$h'^2-h^2=k'^2-k^2;\ h''^2-h^2=k''^2-k^2.$$

Again, as in the former part of this Paper, let

$$x=\mathrm{R}\,\mu' \qquad\qquad a=r\,\mu$$

$$y=\mathrm{R}\sqrt{1-\mu'^2}.\ \mathrm{Sin.}\ \varpi', \qquad b=r\sqrt{1-\mu^2}\ \mathrm{Sin.}\ \varpi$$

$$z=\mathrm{R}\sqrt{1-\mu'^2}.\ \mathrm{Cos.}\ \varpi', \qquad c=r\sqrt{1-\mu^2}\ \mathrm{Cos.}\ \varpi;$$

then R and r are respectively radii of the two ellipsoids.

Farther, assume

$$\frac{h}{k}\,x=\frac{h}{k}\,\mathrm{R}\,\mu'=\mathrm{R}'\,p'$$

$$\frac{h'}{k'}\,y=\frac{h'}{k'}\,\mathrm{R}\sqrt{1-\mu'^2}\ \mathrm{Sin.}\ \varpi'=\mathrm{R}'\sqrt{1-p'^2}\ \mathrm{Sin.}\ q'$$

$$\frac{h''}{k''}\,z=\frac{h''}{k''}\,\mathrm{R}\sqrt{1-\mu'^2}\ \mathrm{Cos.}\ \varpi'=\mathrm{R}'\sqrt{1-p'^2}\ \mathrm{Cos.}\ q'$$

$$\frac{k}{h}\,a=\frac{k}{h}\,r\,\mu=r'\,p$$

$$\frac{k'}{h'}\,b=\frac{k'}{h'}\,r\sqrt{1-\mu^2}\ \mathrm{Sin.}\ \varpi=r'\sqrt{1-p^2}\ \mathrm{Sin.}\ q$$

$$\frac{k''}{h''}\,c=\frac{k''}{h''}\,r\sqrt{1-\mu^2}\ \mathrm{Cos.}\ \varpi=r'\sqrt{1-p^2}\ \mathrm{Cos.}\ q.$$

From these formulæ we get

$$\mathrm{R}'^2=\frac{h^2}{k^2}\,x^2+\frac{h'^2}{k'^2}\,y^2+\frac{h''^2}{k''^2}\,z^2,$$

$$\mathrm{R}^2=x^2+y^2+z^2;$$

and hence,

$$\mathrm{R}'^2-\mathrm{R}^2=\left\{\frac{x^2}{k^2}+\frac{y^2}{k'^2}+\frac{z^2}{k''^2}\right\}(h^2-k^2)=h^2-k^2.$$

In like manner, it is shown that

$$r^2-r'^2=h^2-k.$$

Wherefore,

$$\mathrm{R}^2+r^2=\mathrm{R}'^2+r'^2.$$

And again,

$$a x + b y + c z = R r \gamma = R' r' \gamma';$$

$$\gamma = \mu \mu' + \sqrt{1 - \mu^2} . \sqrt{1 - \mu'^2} \text{ Cos. } (\varpi - \varpi')$$

$$\gamma' = p p' + \sqrt{1 - p^2} . \sqrt{1 - p'^2} \text{ Cos. } (q - q').$$

Wherefore,

$$R^2 - 2 R r \gamma + r^2 = R'^2 - 2 R' r' \gamma' + r'^2.$$

Farther, from the assumed equations we readily derive these values, viz.

$$\text{Tan. } \varpi' = \frac{h''}{h'} . \frac{k'}{k''} . \text{ Tan. } q',$$

$$\frac{\mu'}{\sqrt{1 - \mu'^2}} = \frac{p'}{\sqrt{1 - p'^2}} . \frac{k}{h} . \frac{h' h''}{M},$$

$$M = \sqrt{k'^2 h''^2 \text{ Sin.}^2 q' + h'^2 k''^2 \text{ Cos.}^2 q'}$$

$$R \sqrt{1 - \mu'^2} = R' \sqrt{1 - p'^2} . \frac{M}{k' h''}.$$

And, if we now take the fluxions of the first and second of these formulæ; observing, in the second operation, to make q', and consequently M, constant; we shall get,

$$d \varpi' = d q' . \frac{k' k'' . h' h''}{M^2},$$

$$\frac{d \mu'}{(1 - \mu'^2)^{\frac{3}{2}}} = \frac{d p'}{(1 - p'^2)^{\frac{3}{2}}} . \frac{k h' h''}{h M}$$

$$R^3 (1 - \mu'^2)^{\frac{3}{2}} = R'^3 (1 - p'^2)^{\frac{3}{2}} . \frac{M^3}{k'^3 h''^3}.$$

And, by combining these formulæ, we obtain,

$$R^3 d \mu' d \varpi' = \frac{k k' k''}{h h' h''} . R'^3 d p\ d q'.$$

But, in § 3 it has been shown, that,

$$-\left(\frac{d . \frac{V(r)}{r^2}}{d r}\right) r^3 = \iint \frac{R^2 d \mu' d \varpi'}{\sqrt{R^2 - 2 R r \gamma + r^2}}:$$

and hence, by substitution,

$$-\left(\frac{d . \frac{V(r)}{r^2}}{d r}\right) r^3 = \frac{k k' k''}{h h' h''} . \iint \frac{R'^3 d p' d q'}{\sqrt{R'^2 - 2 R' r' \gamma' + r'^2}}.$$

Now, in the equation of the ellipsoid of which k, k', k'' are the axes, if we substitute $\frac{R' p'}{k}$, $\frac{R' \sqrt{1-p'^2}\,\text{Sin.}\, q'}{k'}$, $\frac{R' \sqrt{1-p'^2}\,\text{Cos.}\, q'}{k''}$ for the equivalent quantities, $\frac{x}{k}$, $\frac{y}{k'}$, $\frac{z}{k''}$; we shall obtain,

$$\frac{1}{R'^2} = \frac{p'^2}{k^2} + \frac{(1-p'^2)\,\text{Sin.}^2 q'}{k'^2} + \frac{(1-p'^2)\,\text{Cos.}^2 q'}{k''} .$$

And, by a like procedure in the other ellipsoid, we obtain,

$$\frac{1}{r'^2} = \frac{p^2}{k^2} + \frac{(1-p^2)\,\text{Sin.}^2 q}{k'^2} + \frac{(1-p^2)\,\text{Cos.}^2 q}{k''^2} .$$

Thus R' is a radius of the ellipsoid that passes through the attracted point, and r' a radius of the given ellipsoid which is entirely within the first figure. The last integral may therefore be developed in a series of the ascending powers of r': and then, applying the same reasoning as in the former case of the developement of Q,* it will be found that all the terms are evanescent, except the first. Thus the general term of the expansion is,

$$r'^i \times \iint \frac{\mathfrak{C}^{(i)} dp'\, dq'}{R'^{i-2}} :$$

and, when i is an odd number, this integral is equal to zero; because the increment at any point on the surface of the sphere, is just equal to the decrement at the point diametrically opposite: and, when i is an even number, it is evanescent; because $\frac{1}{R'^{i-2}}$ contains no terms except such as are evanescent in the integral, according to the theorem in § 5. Wherefore, the integral being reduced to the first term of its expansion, we get,

$$-\left(\frac{d \,.\, \frac{V(r)}{r^3}}{dr}\right) r^3 = \iint R'^3\, dp'\, dq' ;$$

p' and q' being taken between the same limits, as μ' and ϖ'.

* Section 5.

Now, we have

$$R'^2 = \frac{h^2}{p'^2 + \frac{h^2}{h'^2}(1 - p'^2)\,\text{Sin.}^2 q' + \frac{h^2}{h''^2}(1 - p'^2)\,\text{Cos.}^2 q'};$$

or, which is the same thing,

$$R'^2 = \frac{h^2 h'^2 h''^2}{h''^2 (h^2 + e^2 p'^2)\,\text{Sin.}^2 q' + h'^2 (h^2 + e'^2 p'^2)\,\text{Cos.}^2 q'},$$

$$h'^2 - h^2 = e^2,\ h''^2 - h^2 = e'^2;$$

wherefore

$$\iint R'^2\, dp'\, dq' = \iint \frac{h^2 h'^2 h''^2 .\, dp'\, dq'}{h''^2 (h^2 + e^2 p'^2)\,\text{Sin.}^2 q' + h'^2 (h^2 + e'^2 p'^2)\,\text{Cos.}^2 q'}.$$

This expression is now integrable with regard to q': and we get, between the limits $q' = o$ and $q' = 2\pi$,

$$\iint R'^2\, dp'\, dq' = 2\pi .\, h h' h'' . \int \frac{h\, dp'}{\sqrt{(h^2 + e^2 p'^2) . (h^2 + e'^2 p'^2)}}.$$

The integral now found increases as much, while p' decreases from 1 to o, as it does, while p' decreases from o to -1: wherefore the whole value will be the same, if we make p' vary between the limits 1 and o; and then double the result: thus,

$$\iint R'^2\, dp'\, dq' = 4\pi .\, h h' h'' . \int \frac{h\, dp'}{\sqrt{(h^2 + e^2 p'^2) . (h^2 + e'^2 p'^2)}}.$$

Finally put $\frac{p'}{h} = \frac{1}{x}$; then we get,

$$\iint R'^2\, dp'\, dq' = 4\pi .\, h h' h'' . \int \frac{-dx}{\sqrt{(x^2 + e^2)(x^2 + e'^2)}};$$

the limits of x being, $x = h$ and $x = \infty$.

Wherefore, by substitution, we get,

$$-\left(\frac{d . \frac{V(r)}{r^3}}{dr}\right) r^3 = 4\pi .\, k k' k'' . \int \frac{-dx}{\sqrt{(x^2 + e^2)(x^2 + e'^2)}}.$$

Now, multiply by $-\frac{dr}{r^3}$, and then integrate; and, having multiplied by r^3 after the integration, we shall obtain,

$$V(r) = 2\pi .\, k k' k'' . \int \frac{-dx}{\sqrt{(x^2 + e^2)(x^2 + e'^2)}}$$

$$- 2\pi .\, k k' k'' .\, r^3 . \int \frac{1}{r^3} . \frac{-dx}{\sqrt{(x^2 + e^2)(x^2 + e'^2)}}.$$

In the expression under the sign of integration, r increases from its value at the given attracted point till it becomes infinitely great; the angles which it makes with the axes of the co-ordinates remaining constantly the same. But if we substitute the values of a, b, c in the equation,

$$\frac{a^2}{k^2} + \frac{b^2}{k'^2} + \frac{c^2}{k''^2} = 1;$$

we shall get,

$$\frac{1}{r^2} = \frac{\mu^2}{h^2} + \frac{(1-\mu^2)\,\text{Sin.}^2\,\varpi}{h^2 + e^2} + \frac{(1-\mu^2)\,\text{Cos.}^2\,\varpi}{h^2 + e'^2};$$

and, by writing x^2 for h^2,

$$\frac{1}{r^2} = \frac{\mu^2}{x^2} + \frac{(1-\mu^2)\,\text{Sin.}^2\,\varpi}{x^2 + e^2} + \frac{(1-\mu^2)\,\text{Cos.}^2\,\varpi}{x^2 + e'^2};$$

in which expression, μ and ϖ remaining the same, x will vary from h to be infinite, while r increases from its value at the given attracted point to be infinite. Wherefore, by substitution, we get,

$$\begin{aligned}
V(r) = {} & 2\,\varpi\,.\,k\,k'\,k''.\int\frac{-dx}{(x^2+e^2)^{\frac{1}{2}}(x^2+e'^2)^{\frac{1}{2}}} \\
& - 2\,\pi\,.\,k\,k'\,k''.\,r^2\mu^2.\int\frac{-dx}{x^2(x^2+e^2)^{\frac{1}{2}}(x^2+e'^2)^{\frac{1}{2}}} \\
& - 2\,\pi\,.\,k\,k'\,k''.\,r^2(1-\mu^2)\,\text{Sin.}^2\,\varpi.\int\frac{-dx}{(x^2+e^2)^{\frac{3}{2}}(x^2+e'^2)} \\
& - 2\,\pi\,.\,k\,k'\,k''.\,r^2(1-\mu^2)\,\text{Cos.}^2\,\varpi.\int\frac{-dx}{(x^2+e^2)^{\frac{1}{2}}(x+e'^2)^{\frac{3}{2}}}.
\end{aligned}$$

Let M denote the mass of the ellipsoid, then $M = \frac{4}{3}\pi\,.\,k\,k'\,k''$: wherefore, by substituting a, b, c, for the equivalent quantities, we finally get,

$$\begin{aligned}
V(r) = {} & \frac{3M}{2}.\int\frac{-dx}{(x^2+e^2)^{\frac{1}{2}}(x^2+e'^2)^{\frac{1}{2}}} \\
& - a^2.\frac{3M}{2}.\int\frac{-dx}{x^2(x^2+e^2)^{\frac{1}{2}}(x^2+e'^2)^{\frac{1}{2}}} \\
& - b^2.\frac{3M}{2}.\int\frac{-dx}{(x^2+e^2)^{\frac{3}{2}}(x+e'^2)^{\frac{1}{2}}} \\
& - c^2.\frac{3M}{2}.\int\frac{-dx}{(x^2+e^2)^{\frac{1}{2}}(x^2+e'^2)^{\frac{3}{2}}}.
\end{aligned}$$

To this expression we must join the equation of the surface of the ellipsoid that passes through the attracted point, viz.

$$\frac{a^2}{h^2} + \frac{b^2}{h^2 + e^2} + \frac{c^2}{h^2 + e'^2} = 1,$$

by means of which h, the limit of x in the several integrals, is to be determined. When the attracted point is in the surface of the given ellipsoid, it is plain that $h = k$; and the limit of x is, therefore, one of the semi-axes.

Thus an expression of $V(r)$ has been found, that is general for all positions of the attracted point; nothing more being requisite than to determine the limit of x in every particular case. The several integrals are closely connected with one another; they are in forms well known to geometers, and susceptible of many transformations; but, in a general solution, it seems most simple to leave the expression as it is above exhibited.

But although a general expression of $V(r)$ has been found, yet it does not immediately make known the attractive forces acting upon a point. These forces, estimated in directions parallel to the co-ordinates, are represented by the partial fluxions of $V(r)$ relatively to the co-ordinates; but, in performing the operations, it must be observed that x is a function of r, and consequently of the co-ordinates. Thus the attractions of the ellipsoid, respectively parallel to a, b, c, are equal to,

$$-\frac{d.V(r)}{da} - \frac{d.V(r)}{dx} \cdot \frac{dx}{da}$$

$$-\frac{d.V(r)}{db} - \frac{d.V(r)}{dx} \cdot \frac{dx}{db}$$

$$-\frac{d.V(r)}{dc} - \frac{d.V(r)}{dx} \cdot \frac{dx}{dc}.$$

But, according to the foregoing value of $V(r)$,

$$\frac{d.V(r)}{dx} = \frac{\frac{3}{2}M}{\sqrt{(x^2+e^2)(x^2+e'^2)}} \cdot \left\{-1 + \frac{a^2}{x^2} + \frac{b^2}{x^2+e^2} + \frac{c^2}{x^2+e'^2}\right\}:$$

and as this quantity is to be valued at the limit, or when $x = h$, we have $\frac{d \cdot V(r)}{dx} = o$. Wherefore the expressions of the attractive forces are reduced to, $-\frac{d \cdot V(r)}{da}$, $-\frac{d \cdot V(r)}{dl}$ $-\frac{d \cdot V(r)}{dc}$; that is, to the partial fluxions of $V(r)$, supposing that x is independent of the co-ordinates, a, b, c.

The oblate ellipsoid of revolution, corresponds to the supposition, $e^2 = e'^2$; and, in this case, we get,

$$V(r) = \tfrac{3}{2} M \cdot \int \frac{-dx}{x^2 + e^2} - a^2 \cdot \tfrac{3}{2} M \cdot \int \frac{-dx}{x^2 (x^2 + e^2)}$$

$$- (b^2 + c^2) \cdot \tfrac{3}{2} M \cdot \int \frac{-dx}{(x^2 + e^2)^{\frac{3}{2}}}:$$

the equation for finding h, the limit of x, being,

$$\frac{a^2}{h^2} + \frac{b^2 + c^2}{h^2 + e^2} = 1.$$

And, when the attracted point is in the surface of the oblate spheroid of revolution, h is equal to k; and, if we put $\lambda = \frac{e}{k}$, we get,

$$V(r) = \frac{3M}{2k} \cdot \frac{\text{Arc. Tan.} \lambda}{\lambda} - a^2 \cdot \frac{3M}{2k^3} \cdot \frac{\lambda - \text{Arc. Tan.} \lambda}{\lambda^3} - (b^2 + c^2) \cdot \frac{3M}{4k^3} \cdot \frac{\text{Arc. Tan.} \lambda - \frac{\lambda}{1 + \lambda^2}}{\lambda^3}.$$

7. It may not be improper to apply the foregoing solution to find the relation between the figure and the velocity of rotation in the case of an oblate ellipsoid of revolution. As it has been proved that the supposed figure will satisfy one of the conditions of equilibrium,* nothing more is requisite than to employ the other condition, namely, that contained in the equation (A), to determine the relation sought.

Let k be the semi-axis of revolution, and $\sqrt{k^2 + e^2}$, the radius of the equator; if a, b, c be three rectangular co-ordinates of a point in the surface, a being parallel to k, the equation of the figure will be,

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$$\frac{a^2}{k^2} + \frac{b^2 + c^2}{k^2 + e^2} = 1.$$

Put $a^2 = R^2 \mu^2$; $b^2 + c^2 = R^2 (1 - \mu^2)$; $e^2 = k^2 \times \lambda^2$; then, by substitution, we shall get,

$$R^2 (1 + \lambda^2 \mu^2) = k^2 (1 + \lambda^2).$$

Again, from the formula (A), we get

$$V(R) + R^2 . \frac{\omega}{2} (1 - \mu^2) = C,$$

ω being the centrifugal force at the distance 1 from the axis of rotation. This equation must be made identical with the former one, and for this purpose we must substitute in it the value of $V(R)$ reduced to a proper form. Now, M being the mass of the spheroid, we have

$$M = \frac{4\pi}{3} k (k^2 + e^2) = \frac{4\pi}{3} k^3 (1 + \lambda^2);$$

and for the sake of abridging, if we put,

$$A = \frac{\lambda - \text{Arc. Tan.}\,\lambda}{\lambda^3}$$

$$B = \frac{\text{Arc. Tan.}\,\lambda - \frac{\lambda}{1 + \lambda^2}}{2\lambda^3};$$

and likewise attend to the values of a^2 and $b^2 + c^2$, we shall get by the formula in § 6,

$$V(R) = 2\pi k^2 . \frac{(1+\lambda^2)\,\text{Arc. Tan.}\,\lambda}{\lambda} - R^2 . 2\pi \left\{ (1 + \lambda^2) B + (1 + \lambda^2)(A - B) . \mu^2 \right\}:$$

and hence, by substitution, the equation we are considering, when brought to the same form of expression as the first one, will become,

$$R^2 . \left\{ 1 + \frac{(1 + \lambda^2)(A - B) + \frac{\omega}{4\pi}}{(1 + \lambda^2) B - \frac{\omega}{4\pi}} . \mu^2 \right\} = \frac{k^2 (1 + \lambda)^2 \frac{\text{Arc. Tan.}\,\lambda}{\lambda} - \frac{C}{2\pi}}{(1 + \lambda^2) B - \frac{\omega}{4\pi}}.$$

By comparing the two equations, it will appear that the only condition necessary to make them identical, is this, viz.

$$\frac{(1+\lambda^2)(A-B)+\frac{\omega}{4\pi}}{(1+\lambda^2)\,B-\frac{\omega}{4\pi}}=\lambda^2;$$

for the terms on the right-hand sides will be made to coincide by giving a proper value to the arbitrary quantity C. Hence,

$$\frac{\omega}{4\pi}=(1+\lambda^2)\,B-A.$$

Now put,

$$\frac{\omega}{\frac{4}{3}\pi}=q;$$

then, restoring the values of A and B, we shall obtain,

$$\frac{2}{9}q=\frac{1}{3}-\left(\frac{1}{3}+\frac{1}{\lambda^2}\right)\cdot\left(1-\frac{\text{Arc. Tan. }\lambda}{\lambda}\right).$$

From this formula it appears that $q=o$, both when λ is equal to zero and when it is infinitely great. There is therefore no rotatory motion in either of the extreme cases, when the oblateness is nothing, and when it is infinite; or when the fluid mass is a sphere, and when it is a circular sheet spread out in the plane of the equator. In order to discover whether q is evanescent in any other circumstances, put Tan. $\phi=\lambda$;

then $$\frac{2}{9}q=\frac{1}{3}-\left(\frac{1}{\text{Sin.}^2\phi}-\frac{2}{3}\right)\left(1-\frac{\phi\,\text{Cos.}\,\phi}{\text{Sin.}\,\phi}\right);$$

or, in a series,

$$q=\frac{2}{5}\,\text{Sin.}^2\phi+\frac{1.2}{5.7}\,\text{Sin.}^4\phi-\frac{1.2.4.6}{5.7.9.11}\,\text{Sin.}^8\phi$$
$$-\frac{1.2.4.6.8}{5.7.9.11.13}\,\text{Sin.}^{10}\phi$$
$$-\&c.$$

Now this series being evanescent both when Sin. $\phi=o$, and when Sin. $\phi=1$, it follows that, for every other value of Sin. ϕ, q will be positive; and hence it will first increase from zero to a maximum, and then decrease to the first limit. If

we seek the value of λ that will make q a maximum, we shall find this equation, viz.

$$\text{Arc. Tan. } \lambda = \frac{\lambda}{1+\lambda^2} \cdot \frac{9+7\lambda^2}{9+\lambda^2};$$

from which λ comes out equal to 2·5292. And hence $\sqrt{1+\lambda^2}$, which is the proportion of the equatorial diameter to the polar axis, is equal to 2.7197.

From all this it follows, that if a homogeneous mass of fluid in equilibrio, at rest, and consequently of a spherical figure, begin to revolve about a diameter, it will become more and more oblate as the velocity of rotation increases, till the equatorial diameter have to the polar axis the proportion of 2·7197 to 1: arrived at this point the rotatory velocity must decrease, in order that the fluid in equilibrio continue to have the figure of an ellipsoid of revolution with increasing oblateness; in so much that while the oblateness tends to be infinite, and the fluid to become a circular sheet in the plane of the equator, the velocity of rotation continually approaches to zero.

As the oblateness increases without ceasing, there is but one rotatory velocity with which a spheroid of a given figure will be in equilibrio.

But when a fluid mass is to revolve in a given time, and the figure that will maintain the equilibrium is sought, there are two solutions, if the proposed rotation be within the maximum, and one only, when it reaches that limit.

When the rotatory velocity is greater than the maximum, the equilibrium cannot take place: for, on the one hand, the proposed rotation is inconsistent with the figure of an ellipsoid; and, on the other, it has been proved, that a homoge-

neous fluid cannot be in equilibrio unless it have that figure. In this case, therefore, the fluid would first extend itself, and flatten to a certain degree with a decreasing velocity of rotation, and then oscillate back with an increasing rotatory motion. But the tenacity of the particles would gradually diminish, and finally destroy, the oscillations of the fluid; which would therefore ultimately settle in one of the figures of equilibrium; that is, in an elliptical spheroid of revolution having the equatorial diameter more than 2·7179 times the axis of revolution.

When the oblate figures are little different from spheres, as in the case of the planets, λ, which is equal to the excentricity of the meridian divided by half the polar axis, is so small that we may consider λ^2 as equal to $\text{Sin.}^2\,\phi$, and may reject all the powers of these two quantities. The series for q will thus be reduced to its first term, viz.

$$q = \frac{2}{5}\,\text{Sin.}^2\,\phi = \frac{2}{5}\,\lambda^2.$$

But the polar axis is to the equatorial diameter as 1 to $\sqrt{1+\lambda^2}$, or as 1 to $1+\frac{1}{2}\lambda^2$: wherefore the same proportion is equal to that of 1 to $1+\frac{5}{4}q$.

Again, we have $$q = \frac{\omega}{\frac{4}{3}\pi};$$

now, $\frac{4}{3}\pi$ being the mass of a sphere of which the density and the radius are each equal to unit, it will represent the gravitation at the surface; and, if we suppose the same sphere to revolve with the given rotatory velocity, ω will be the centrifugal force at the equator. Wherefore q is the proportion of the centrifugal force at the equator to the gravity; a proportion which remains the same in all spheres that have the

same density and the same velocity of rotation, because both the quantities increase as the radius of the sphere. Hence, in a planet of small oblateness, the value of q to the degree of approximation mentioned, is equal to the proportion of the centrifugal force to the gravitation at the equator; and the proportion of 1 to $1 + \frac{5}{4} q$ is equal to that of the polar axis to the equatorial diameter.

8. In the determination of the equilibrium of a homogeneous fluid mass investigated in this Paper, two conditions are found necessary when the particles are endowed with attractive powers; whereas, in the usual solution of the problem one only is deemed sufficient, namely, that contained in equation (A), which expresses that the resultant of the accelerating forces acting upon the particles in the outer surface shall be every where perpendicular to that surface. It is extremely remarkable that, of the two conditions, the one which is usually omitted, alone and without reference to the other, ascertains the kind of the figures of equilibrium.

M'LAURIN first proved synthetically that the ellipsoid, whatever be the degree of oblateness, fulfils all the conditions requisite for maintaining the equilibrium of a homogeneous fluid mass that revolves about an axis. If therefore the equation (A) were alone sufficient for the equilibrium, the ellipsoid must be deducible from it, not in particular suppositions and approximately, but generally, and by an accurate process of reasoning. But this has not been accomplished, nor even attempted, by any geometer. No application has hitherto been made of the hydrostatical theory, except in the case of spheroids little different from spheres.

If a homogeneous fluid of a spherical form at rest, and

consequently in equilibrio, begin to revolve about a diameter with a rotatory velocity causing a centrifugal force at the equator, very small in proportion to the gravitation; the sphere will acquire a small degree of oblateness at the poles, and the new surface of equilibrium must come under the equation (A). Now, from these considerations alone, without any reference to the other condition of equilibrium, it has been proved by LEGENDRE and LAPLACE, that the particles of the fluid will arrange themselves very nearly in an ellipsoid of revolution, the deviation being proportional to the square and higher powers of the oblateness. But, as the coincidence of the true figure of equilibrium with the ellipsoid is not exact, the result seems to be inconsistent with what M'LAURIN has so ably and elegantly demonstrated to be true. This argument will acquire greater force, and will even become conclusive, against the theory which makes the equation (A) the only condition of the equilibrium, if we consider the oblateness as a finite quantity, and push the approximation so as to take in the square and higher powers:* for, by this procedure, we obtain a series of figures in which the ellipsoid is not included.

There is a great analogy between the modern theory of spheroids little different from spheres, and the assumption of NEWTON, who tacitly supposed that the fluid sphere, in the nascent change of its form, will become, either exactly or very nearly, an elliptical spheroid, oblate at the poles. Both views of the subject leave us in ignorance of the exact form of the surface of equilibrium, although, in the supposed circumstances, it is proved in the one, and assumed in the other, that it is nearly an ellipsoid.

* Mec. Celeste, Vol. ii, p. 105, No. 37.

The figures of the earth and of the planets being entirely deduced from the properties of spheroids little different from spheres, it may not be improper to conclude this Paper with a short exposition of a theory that occupies so conspicuous a place in the celestial mechanics, and which is so intimately connected with the subject we have been discussing.

For this purpose resume the expansion of $V(r)$ already given in § 3, viz.

$$\begin{aligned} V(r) = & \iint R'^2 \, d\mu' \, d\varpi' + r \iint R' . C^{(1)} \, d\mu' \, d\varpi' \\ & + r^2 . \left\{ -\frac{2\pi}{3} + \iint \text{Log.} \, R' . C^{(2)} \, d\mu' \, d\mu' \right\} \\ & - \frac{r^3}{1} \iint \frac{C^{(3)} \, d\mu' \, d\varpi'}{R'} \\ & \quad \vdots \\ & - \frac{r^i}{i-2} \iint \frac{C^{(i)} \, d\mu' \, d\varpi'}{R'^{i-2}} \\ & \quad \&c. \end{aligned}$$

The spheroid being nearly a sphere, we may suppose $R' = a . (1 + \alpha . y')$; α being a small coefficient of which the square and other powers are to be neglected; and y' a function of the angles that determine the position of R'. The expansion supposes that the attracted point is within the spheroid; but it will apply when the same point is in the surface, in which case, $r = R = a(1 + \alpha . y)$. Now, let the values of R and R' be substituted, and we shall obtain,

$$\begin{aligned} V(R) = & \iint \frac{a^2}{2} (1 + 2\alpha y') \, d\mu' \, d\varpi' - \frac{2\pi}{3} a^2 (1 + 2\alpha y) \\ & + a^2 \alpha . \iint y' . C^{(1)} \, d\mu' \, d\varpi' \\ & + a^2 \alpha . \iint y' . C^{(2)} \, d\mu' \, d\varpi' \\ & + a^2 \alpha . \iint y' . C^{(3)} \, d\mu' \, d\varpi' \\ & + \&c. \end{aligned}$$

This expression is to be substituted in the equation (A): and it is to be observed that $\iint \frac{a^2}{2} d\mu' d\varpi' = 2\pi a^2$; and that ω is of the same order with α. Hence we get,

$$\begin{aligned} C = \tfrac{4\pi}{3} a^2 - \tfrac{4\pi}{3} a^2 y &+ a^2 \alpha . \iint y' \, d\mu' d\varpi' \\ &+ a^2 \alpha . \iint y' . C^{(1)} d\mu' d\varpi' \\ &+ a^2 . \left\{ \alpha . \iint y' . C^{(2)} d\mu' d\varpi' + \tfrac{\omega}{2} (1 - \mu^2) \right\} \\ &+ a^2 \alpha . \iint y' . C^{(3)} d\mu' d\varpi' \\ &+ \text{\&c.} \end{aligned}$$

This is the approximate equation of the surface, when the equation (A) is alone taken into account; and it is to be proved that this equation cannot subsist unless it belong to an elliptical spheroid of revolution.

In the first place, the nature of the function y must depend upon the integrals by which its value is expressed. But all the integrals are independent of the angles that determine the position of the attracted point in the surface, unless so far as those angles enter into the expressions $C^{(1)}$, $C^{(2)}$, $C^{(3)}$, &c. which are all functions of γ. Now γ is a function of μ, $\sqrt{1-\mu^2}$ Sin. ϖ, $\sqrt{1-\mu^2}$. Cos. ϖ: and hence it follows, that y and y' are functions of three rectangular co-ordinates of a point in the surface of a sphere.

In the second place, every function of three rectangular co-ordinates is susceptible of an arrangement, by which it will be converted into a series of the same integrals contained in the foregoing equation. Let

$$f = \sqrt{1 - 2\epsilon\gamma + \epsilon^2}:$$

then, as is well known, we shall have,

$$4\pi y = \iint \left(\frac{1}{f} + 2\iota \frac{d.\frac{1}{f}}{d\iota} \right) y' \, d\mu' \, d\varpi',$$

provided we make $\iota = 1$, after the integration. Now for $\frac{1}{f}$ substitute its developement,* and make $\iota = 1$: then,

$$\begin{aligned} 4\pi y = & \iint y' \, d\mu' \, d\varpi' \\ & + 3\iint y' . C^{(1)} \, d\mu' \, d\varpi' \\ & + 5\iint y' . C^{(2)} \, d\mu' \, d\varpi' \\ & \quad \cdot \\ & \quad \cdot \\ & \quad \cdot \\ & + (2i+1)\iint y' . C^{(i)} \, d\mu' \, d\varpi' \\ & + \&c. \end{aligned}$$

This expression is identical when y and y' are functions of three rectangular co-ordinates. It is analytically true of every function that can be algebraically transformed into an expression of three rectangular co-ordinates; and thus it may be said to comprehend every function of two variable angles.

We have now obtained two expressions of y in the same quantities. But it is easy to prove that the same function can be so expressed only one way. The two values of y must therefore be identical; and all the terms that cannot be made to coincide, must be evanescent. Hence we obtain

$$\begin{aligned} o &= \iint y' . C^{(1)} \, d\mu' \, d\varpi' \\ o &= \iint y' . C^{(3)} \, d\mu' \, d\varpi' \\ & \quad \cdot \\ & \quad \cdot \\ & \quad \cdot \\ o &= \iint y' . C^{(i)} \, d\mu' \, d\varpi' \\ & \quad \&c. \end{aligned}$$

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from which it is easy to infer that the most general value of y' is thus expressed, viz.

$$y' = A \cdot \mu'^2 + B(1-\mu'^2)\,\text{Sin.}^2\varpi' + C(1-\mu'^2)\,\text{Cos.}^2\varpi'.$$

It deserves to be remarked, that the equations just found are the very same that result from the second condition of equilibrium, when, for R', we substitute $a(1+\alpha \cdot y')$, and neglect the powers of α.

Now, leaving out the evanescent terms, the two foregoing expressions will become,

$$C = \frac{4\pi}{3}a^2 - \frac{4\pi}{3}a^2 y + a^2\alpha \cdot \int\int y'\,d\mu'\,d\varpi'$$
$$+ a^2 \cdot \left\{\alpha\int\int y' \cdot C^{(2)}\,d\mu'\,d\varpi' + \frac{\omega}{2}(1-\mu^2)\right\}$$
$$4\pi y = \int\int y'\,d\mu'\,d\varpi' + 5\int\int y' \cdot C^{(2)}\,d\mu'\,d\varpi':$$

and farther, if we exterminate the integral containing $C^{(2)}$ from the first, we shall obtain;

$$C = \frac{4\pi}{3}a^2 + \frac{4}{5}a^2\alpha \cdot \int\int y'\,d\mu'\,d\varpi'$$
$$- a^2 \cdot \left\{\frac{8\pi}{15} \cdot \alpha y - \frac{\omega}{2}(1-\mu^2)\right\}$$
$$4\pi y = \int\int y'\,d\mu'\,d\varpi' + 5 \cdot \int\int y' \cdot C^{(2)}\,d\mu'\,d\varpi'.$$

The first of these equations proves that y is a function of μ only, and that the spheroid sought is one of revolution. The second is satisfied by putting,

$$y = f(1-\mu^2)$$
$$y' = f(1-\mu'^2):$$

wherefore, by substituting these values, the first will become,

$$C = \frac{4\pi}{3}a^2\left(1+\tfrac{8}{5}\alpha f\right) - a^2\left(\frac{8\pi}{15} \cdot \alpha f - \frac{\omega}{2}\right)(1-\mu^2).$$

Hence we finally get,

$$q = \frac{\omega}{\frac{4}{3}\pi}$$

$$\alpha f = \frac{5}{4} q$$

$$C = \frac{4\pi}{3} a^3 . (1 + 2q)$$

$$\alpha y = \alpha f (1 - \mu^2) = \frac{5}{4} q (1 - \mu^2)$$

$$a (1 + \alpha y) = a \left\{ 1 + \frac{5}{4} q (1 - \mu^2) \right\}.$$

Such is the method of investigation for which we are indebted to LEGENDRE and LAPLACE in its fundamental principles: for, when all the operations necessary for applying it extensively and readily are fully explained, it becomes a great branch of analysis. The result is no more than an approximation, both on account of the quantities omitted, and because no attention is paid to one of the conditions of equilibrium. Considering the near approach of all the planets to the spherical form, the method of calculation may be deemed sufficiently accurate for determining the figure of the fluids that cover their surfaces; but it is not the less necessary to place the physical theory on a clear and sure foundation. As the subject is usually treated, there is an obscurity, and a want of evidence, arising from the inconsistency between the hydrostatical theory and what is proved by M'LAURIN, which is extremely embarrassing, but which entirely disappears, when we take into account all the physical conditions requisite to maintain the equilibrium of a homogeneous fluid mass that revolves upon an axis.

J. IVORY.

November 2, 1823.

VI. *On the corrosion of copper sheeting by sea water, and on methods of preventing this effect; and on their application to ships of war and other ships. By Sir* Humphry Davy, *Bart. Pres. R. S.*

Read January 22, 1824.

1. The rapid decay of the copper sheeting of His Majesty's ships of war, and the uncertainty of the time of its duration, have long attracted the attention of those persons most concerned in the naval interests of the country. Having had my enquiries directed to this important object by the Commissioners of the Navy Board, and a Committee of the Royal Society having been appointed to consider of it, I entered into an experimental investigation of the causes of the action of sea water upon copper. In pursuing this investigation, I have ascertained many facts which I think not unworthy of the notice of the Royal Society, as they promise to illustrate some obscure parts of electro-chemical science; and likewise seem to offer important practical applications.

2. It has been generally supposed that sea water had little or no action on pure copper, and that the rapid decay of the copper on certain ships was owing to its impurity. On trying, however, the action of sea water upon two specimens of copper, sent by John Vivian, Esq. to Mr. Faraday for analysis, I found the specimen which appeared absolutely pure, was acted upon even more rapidly than the specimen which contained alloy: and, on pursuing the enquiry with specimens of various kinds of copper which had been collected by the

Navy Board, and sent to the Royal Society, and some of which had been considered as remarkable for their durability, and others for their rapid decay, I found that they offered very inconsiderable differences only in their action upon sea water; and, consequently, that the changes they had undergone must have depended upon other causes than the absolute quality of the metal.

3. To enable persons to understand fully the train of these researches, it will be necessary for me to describe the nature of the chemical changes taking place in the constituents of sea water by the agency of copper.

When a piece of polished copper is suffered to remain in sea water, the first effects observed are, a yellow tarnish upon the copper, and a cloudiness in the water, which take place in two or three hours: the hue of the cloudiness is at first white; it gradually becomes green. In less than a day a bluish-green precipitate appears in the bottom of the vessel, which constantly accumulates; at the same time that the surface of the copper corrodes, appearing red in the water, and grass-green where it is in contact with air. Gradually carbonate of soda forms upon this grass-green matter; and these changes continue till the water becomes much less saline.

The green precipitate, when examined by the action of solution of ammonia and other tests, appears principally to consist of an insoluble compound of copper, (which may be considered as a hydrated sub-muriate) and hydrate of magnesia.

According to the views which I developed fourteen years ago, of the nature of the compounds of chlorine, and which

are now generally adopted, it is evident that soda and magnesia cannot appear in sea water by the action of a metal, unless in consequence of an absorption or transfer of oxygene. It was therefore necessary for these changes, either that water should be decomposed, or oxygene absorbed from the atmosphere. I found that no hydrogene was disengaged, and consequently no water decomposed: necessarily, the oxygene of the air must have been the agent concerned, which was made evident by many experiments.

Copper in sea water deprived of air by boiling or exhaustion, and exposed in an exhausted receiver or an atmosphere of hydrogene gas, underwent no change; and an absorption in atmospherical air was shown when copper and sea water were exposed to its agency in close vessels.

4. In the Bakerian Lecture for 1806, I have advanced the hypothesis, that chemical and electrical changes may be identical, or dependent upon the same property of matter: and I have farther explained and illustrated this hypothesis in an elementary work on chemistry, published in 1812. Upon this view, which has been adopted by M. Berzelius and some other philosophers, I have shown that chemical attractions may be exalted, modified, or destroyed, by changes in the electrical states of bodies; that substances will only combine when they are in different electrical states; and that, by bringing a body naturally positive artificially into a negative state, its usual powers of combination are altogether destroyed; and it was by an application of this principle that, in 1807, I separated the bases of the alkalies from the oxygene with which they are combined, and preserved them for

examination; and decomposed other bodies formerly supposed to be simple.

It was in reasoning upon this general hypothesis likewise, that I was led to the discovery which is the subject of this Paper.

Copper is a metal only weakly positive in the electrochemical scale; and, according to my ideas, it could only act upon sea water when in a positive state; and, consequently, if it could be rendered slightly negative, the corroding action of sea water upon it would be null; and whatever might be the differences of the kinds of copper sheeting and their electrical action upon each other, still every effect of chemical action must be prevented, if the whole surface were rendered negative. But how was this to be effected? I at first thought of using a Voltaic battery; but this could be hardly applicable in practice. I next thought of the contact of zinc, tin, or iron: but I was for some time prevented from trying this, by the recollection that the copper in the Voltaic battery, as well as the zinc, is dissolved by the action of diluted nitric acid; and by the fear that too large a mass of oxidable metal would be required to produce decisive results. After reflecting, however, for some time on the slow and weak action of sea water on copper, and the small difference which must exist between their electrical powers and knowing that a very feeble chemical action would be destroyed by a very feeble electrical force, I resolved to try some experiments on the subject. I began with an extreme case. I rendered sea water slightly acidulous by sulphuric acid, and plunged into it a polished piece of copper, to which

a piece of tin was soldered equal to about one-twentieth of the surface of the copper. Examined after three days the copper remained perfectly clean, whilst the tin was rapidly corroded: no blueness appeared in this liquor; though, in a comparative experiment, when *copper alone* and the same fluid mixture was used, there was a considerable corrosion of the copper, and a distinct blue tint in the liquid.

If one-twentieth part of the surface of tin prevented the action of sea water rendered slightly acidulous by sulphuric acid, I had no doubt that a much smaller quantity would render the action of sea water, which depended only upon the loosely attached oxygene of common air, perfectly null; and on trying $\frac{1}{200}$ part of tin, I found *the effect* of its preventing the corrosion of the copper perfectly decisive.

5. This general result being obtained, I immediately instituted a number of experiments, in most of which I was assisted by Mr. Faraday, to ascertain all the circumstances connected with the preservation of copper by a more oxidable metal. I found, that whether the tin was placed either in the middle, or at the top, or at the bottom of the sheet of copper, its effects were the same; but, after a week or ten days, it was found that the defensive action of the tin was injured, a coating of sub-muriate having formed, which preserved the tin from the action of the liquid.

With zinc or iron, whether malleable or cast, no such diminution of effect was produced. The zinc occasioned only a white cloud in the sea water, which speedily sunk to the bottom of the vessel in which the experiment was made. The iron occasioned a deep orange precipitate; but, after many weeks, not the smallest portion of copper was found in the

water; and so far from its surface being corroded, in many parts there was a regeneration of zinc or of iron found upon it.

6. In pursuing these researches, and applying them to every possible form and connection of sheet copper, the results were of the most satisfactory kind. A piece of zinc as large as a pea, or the point of a small iron nail, were found fully adequate to preserve forty or fifty square inches of copper; and this, wherever it was placed, whether at the top, bottom, or in the middle of the sheet of copper, and whether the copper was straight or bent, or made into coils. And where the connection between different pieces of copper was completed by wires, or thin filaments of the fortieth or fiftieth of an inch in diameter, the effect was the same; every side, every surface, every particle of the copper remained bright, whilst the iron or the zinc was slowly corroded.

A piece of thick sheet copper, containing on both sides about sixty square inches, was cut in such a manner as to form seven divisions, connected only by the smallest filaments that could be left, and a mass of zinc, of the fifth of an inch in diameter, was soldered to the upper division. The whole was plunged under sea water; the copper remained perfectly polished. The same experiment was made with iron: and now, after a lapse of a month, in both instances, the copper is as bright as when it was first introduced, whilst similar pieces of copper, undefended, in the same sea water, have undergone considerable corrosion, and produced a large quantity of green deposit in the bottom of the vessel.

A piece of iron nail about an inch long was fastened by a piece of copper wire, nearly a foot long, to a mass of sheet

copper, containing about forty square inches, and the whole plunged below the surface of sea water; it was found, after a week, that the copper was defended by the iron in the same manner as if it had been in immediate contact.

A piece of copper and a piece of zinc soldered together at one of their extremities, were made to form an arc in two different vessels of sea water; and the two portions of water were connected together by a small mass of tow moistened in the same water: the effect of the preservation of the copper took place in the same manner as if they had been in the same vessel.

As the ocean may be considered, in its relation to the quantity of copper in a ship, as an infinitely extended conductor, I endeavoured to ascertain whether this circumstance would influence the results; by placing two very fine copper wires, one undefended, the other defended by a particle of zinc, in a very large vessel of sea water, which water might be considered to bear the same relation to so minute a portion of metal as the sea to the metallic sheeting of a ship. The result of this experiment was the same as that of all the others; the defended copper underwent no change; the undefended tarnished, and deposited a green powder.

Small pieces of zinc were soldered to different parts of a large plate of copper, and the whole plunged in sea water: it was found that the copper was preserved in the same manner as if a single piece had been used.

A small piece of zinc was fastened to the top of a plate of polished copper, and a piece of iron of a much larger size was soldered to the bottom, and the combination placed in sea water. Not only was the copper preserved on both sides

in the same manner as in the other experiments, but even the iron; and after a fortnight, both the polish of the copper and the iron remained unimpaired.

7. I am continuing these researches, and I shall communicate such of them as are connected with new facts, to the Royal Society.

The Lords Commissioners of the Admiralty, with their usual zeal for promoting the interests of the Navy by the application of science, have given me permission to ascertain the practical value of these results by experiments upon ships of war; and there seems every reason to expect (unless causes should interfere of which our present knowledge gives no indications) that small quantities of zinc, or which is much cheaper, of malleable or cast iron, placed in contact with the copper sheeting of ships, which is all in electrical connection, will entirely prevent its corrosion. And as negative electricity cannot be supposed favourable to animal or vegetable life; and as it occasions the deposition of magnesia, a substance exceedingly noxious to land vegetables, upon the copper surface; and as it must assist in preserving its polish, there is considerable ground for hoping that the same application will keep the bottoms of ships clean, a circumstance of great importance both in trade and naval war.

It will be unnecessary for me to dwell upon the economical results of this discovery, should it be successful in actual practice, or to point out its uses in this great maritime and commercial country.

I might describe other applications of the principle to the preservation of iron, steel, tin, brass, and various useful metals; but I shall reserve this part of the subject for another communication to the Royal Society.

VII. *A finite and exact expression for the Refraction of an Atmosphere nearly resembling that of the Earth.* By Thomas Young, *M. D. For. Sec. R. S.*

Read February 5, 1824.

It has lately been demonstrated, in the Journal of the Royal Institution, that if the pressure of the atmosphere ,y, be represented either by the square or by the cube of the square root of the density ,z, the astronomical refraction ,r, may be obtained in a finite equation. Mr. Ivory, in a very ingenious and elaborate paper lately presented to the Royal Society, has computed the refraction, by means of several refined transformations, and with the assistance of converging series, from an equation which expresses the pressure in terms of the density and of its square: I have now to observe, that if we substitute, for the simple density, the cube of its square root, and make $y = \frac{3}{2} z^{\frac{4}{3}} - \frac{1}{2} z^{2}$, we shall represent the constitution of the most important part of the atmosphere with equal accuracy, although this expression supposes the total height somewhat smaller than the truth, and belongs to one of those hypotheses, which Mr. Ivory has considered as inadmissible: it has the advantage, however, of affording a direct equation for the refraction, which agrees very nearly with Mr. Ivory's table, and still more accurately with the French table, and with that which has been published for some years in the Nautical Almanac.

Since $\mathrm{d}x = \frac{-\mathrm{d}y}{mz}$, m being the number of times that the modulus of the atmospherical elasticity is contained in the

radius of the earth, and here $dy = \frac{9}{4}\sqrt{z}\,dz - z\,dz$, we have $dx = -\frac{9}{4}\frac{dz}{m\sqrt{z}} + \frac{dz}{m}$, and $\int dx = -\frac{9}{2m}\sqrt{z} + \frac{z}{m} + \frac{7}{2m}$, for the height above the earth's surface, which, when $z = 0$, becomes $\frac{7}{2} \times 27300 = 95550$ feet. For the refraction, we have the equation $dr = \frac{-p\,dz}{\sqrt{(x^2 - s^2 - 2p[1-z])}}$ (Astr. Coll. XV.) $= \frac{-p\,dz}{\sqrt{(2\int dx + v^2 - 2p + 2pz)}}$, which is the value originally assigned to this fluxion by Dr. BROOK TAYLOR; v being the sine of the apparent altitude; and here

$$dr = \frac{-p\,dz}{\sqrt{\left(\frac{7}{m} - \frac{9}{m}\sqrt{z} + \frac{2z}{m} + v^2 - 2p + 2pz\right)}};$$

or, if $\sqrt{z} = \psi$, and $dz = 2\psi\,d\psi$,

$$\frac{dr}{2p} = \frac{\psi\,d\psi}{\sqrt{\left(\frac{7}{m} - 2p + v^2 - \frac{9}{m}\psi + \left[\frac{2}{m} + 2p\right]\psi^2\right)}}.$$

which is equivalent to the $\frac{x\,dx}{\sqrt{(a + bx + cxx)}}$ of the Article FLUENTS in the Encyclopædia Britannica, No. 259; the fluent being

$$\frac{1}{c}\left[\sqrt{(a + bx + cx^2)} - \frac{b}{2\sqrt{c}}\,\mathrm{hl}\,(2cx + b + 2\sqrt{c}\sqrt{[a + bx + cx^2]})\right],$$

and its whole value, from $z = 1$ to $z = 0$, being $-\frac{cr}{2p} = \sqrt{(a' + v^2)} - v + \frac{b}{2\sqrt{c}}\,\mathrm{hl}\,\frac{2c + b + 2v\sqrt{c}}{b + 2\sqrt{c}\sqrt{(a' + vv)}}$, putting $a' = \frac{7}{m} - 2p$, since $a + b + c = v^2$.

For the numerical values of the coefficients, taking, at the temperature of $50°$, $p = .0002835$, and $\frac{1}{m} = .001294 = \frac{1}{772.8}$, $a = \frac{7}{m} - 2p + v^2 = .008491 + v^2$, $b = \frac{-9}{m} = -.011646$, and $c = \frac{2}{m} + 2p = .003155$; hence $\frac{2p}{c} = .17972$, $\sqrt{a'} = .0921466$, $\sqrt{c} = .05617$, $\frac{-b}{2\sqrt{c}} = .10367$, $2c + b = -.005336$, and

$$r = .17972\left(\sqrt{(.008491 + v^2)} - v - .10367\,\mathrm{hl}\,\frac{.005336 - .11234v}{.011646 - .11234\sqrt{(.008491 + vv)}}\right);$$

and at the horizon, when $v = 0$,

$$r = .17972\left(.0921466 - .10367\,\mathrm{h\,l}\,\frac{.005336}{.011646 - .11234 \times .092147}\right) =$$

$.009840 = 33'\,42'',5$; which is only $1'',5$ less than the quantity assigned by the French tables and in the Nautical Almanac, while Mr. Ivory makes it $34'\,17'',5$. Again, if we take $v = .1$, for the altitude $5°\,44'\,21''$, we obtain $8'\,49'',5$ for the refraction, while the Nautical Almanac gives us $8'\,53''$, and Mr. Ivory's table $8'\,49'',6$. There is however no reason for proceeding to compute a new table by this formula, the method employed for the table in the Nautical Almanac being rather more compendious in all common cases: and even if it were desired to represent Mr. Ivory's table by the approximation there employed, we might obtain the same results, with an error never much exceeding a single second, from the equation

$$00028333 = \frac{v}{s}\,r + \frac{2.26 + \frac{1}{2}vv}{ss}\,r^2 + 5400\,\frac{rr}{ss}\left(\frac{v}{s}\,r + \frac{1.13 + \frac{1}{4}vv}{ss}\,r^2\right).$$

Welbeck Street,
3rd. February, 1824.

VIII. *The Bakerian Lecture. On certain motions produced in fluid conductors when transmitting the electric current. By* J. F. W. Herschel, *Esq. F. R. S.*

Read February 12, 1824.

1. Having had occasion, in the course of some enquiries respecting the decomposing agency of the Voltaic pile, to electrify mercury in contact with various saline solutions, I was surprised to observe motions take place in the fluid metal of a violent and apparently capricious kind, for which, as I had uniformly operated with very feeble electric powers, there seemed no adequate cause. Frequently it would be agitated with convulsive starts; sometimes currents and eddies of great violence would be formed in it; at others, it would spread and elongate itself, ramifying out into the most irregular forms; and altogether presenting appearances of a nature so singular, as induced me to make experiments with a view to ascertain their cause, or at least the circumstances essential to their reproduction.

2. The singular convulsive agitations into which mercury is thrown when placed within the circuit of a powerful Voltaic battery discharged through water, has been noticed by Sir H. Davy, in his Elements of Chemical Philosophy. Pure water, however, is so very imperfect a conductor, that great Voltaic powers must be used; and the phænomena are then too irregular, and the agitations too violent for distinctness.

It is only when liquids which conduct well are used to form the circuit, that they become regular, and can be studied at leisure under the influence of moderate electric energies.

3. If a quantity of very pure and perfectly clean mercury, free from the slightest superficial film, be placed in a Wedgewood-ware evaporating basin (which must also be scrupulously clean), and covered to the depth of about a quarter of an inch with concentrated sulphuric acid, and the extremities of two wires of platina in connexion with the poles of a Voltaic* apparatus be immersed *in the acid only* on opposite sides of the mercury, but not in contact with it; immediately a rapid circulation will be seen to take place in the acid, owing to a violent current which establishes itself between the two wires, setting directly across the mercury in a direction from the negative (or zinc) towards the positive (or copper) pole. This current is kept up steadily, and without any change in its direction or force so long as the pile remains in activity, and only flags, and at length ceases, when its energy is quite exhausted. The mercury is not sensibly tarnished or otherwise acted on, and, after the experiment, is found to have undergone no change; nor is the acid sensibly altered, with the exception of the trifling portion decomposed, and a minute quantity of mercury taken up.

4. If we examine the phænomena more attentively, we shall observe that the particles of the acid in immediate contact *with the mercury*, are those which move most actively, being

* The battery I employed in this and the subsequent experiments (unless where the contrary is expressed), consisted of ten pairs of single plates, each of fourteen square inches in surface, excited by mixed nitric and sulphuric acids much diluted.

darted along its surface with surprising violence; those above them, and more remote, appearing rather to be dragged or forced along by them, than impelled by any force acting directly on themselves. We shall perceive too, that, if some distance intervene between the wires and the edges of the mercury, the current will be confined, and the circulation take place in the immediate neighbourhood of the mercury *only*, the liquid around the wires being nearly, or quite at rest.

5. If the centre of the globule or disc of mercury be situated in one straight line with the extremities of the wires, the current will set diametrically across it; but if this be not the case, it will follow a curvilinear course, every elementary filament of it having a different curvature, and each traversing the mercury in a path having a common origin and termination, viz. the points (z) and (c) of its surface nearest to the negative and positive poles respectively.

6. If the globule of mercury be of considerable size (four hundred or five hundred grains for instance), it will be observed to elongate itself in the direction of its axis towards the negative wire, and if near enough, will reach and amalgamate with it: but if it be small, its whole mass will move bodily with more or less rapidity, as if attracted to the negative wire. This apparent attraction is often very energetic, the globule moving with great velocity towards the negative wire, to which it immediately adheres. If the wires form a triangle with the situation of the globule while at rest, the latter advances neither directly to the negative, nor directly from the positive wire, but in a direction oblique to both, approaching the negative wire in a spiral, and describing frequently several revolutions with increasing velocity before it

ultimately falls into and amalgamates with it, like a body acted on at once by an attractive force tending to the negative, and a repulsive, from the positive wire.

7. These apparent attractions and repulsions, this elongation of large masses of mercury and bodily motion of small ones toward the negative pole, are in reality, however, only secondary effects; their immediate cause, as well as that of the currents in the surrounding acid, will be discovered by a more minute attention to what takes place in the mercury itself, while under the influence of the electric action.

8. To this end, if we operate on a considerable mass of mercury, and, instead of covering it with the acid, merely moisten it and the containing vessel, making the circuit as before, only by the medium of the thin film of acid which adheres, the circulation of the mercury will be not less violent; but it will then be evident that the origin of the motion is in the mercury itself, the acid film being (so far as mechanical impulse is concerned) merely passive, and dragged along by its adherence to the mercury, coating it frequently with a stratum so thin as to reflect iridescent colours over its whole surface, and render the phænomenon extremely beautiful. The motion of the mercury consists in a continual radiation of its superficial molecules from the point nearest to the negative pole, by which it is kept in a constant state of circulation, each particle being urged along the surface from the negative to the positive pole and returning along the axis. Were the mercury insulated from contact with the bottom of the sustaining vessel, and devoid of adhesion to the liquid, the momentum of the portions going and

returning would be equal, and the centre of gravity of the whole mass would remain at rest; but by reason of the friction and adhesion of the fluid metal to the vessel and liquid, these re-act on the globule in a direction contrary to that of the superficial currents, and the centre of gravity accordingly advances in that direction, or towards the negative pole. When this motion cannot take place, the internal current, having all one uniform direction, forces its way outwards to the negative pole, distorting and elongating the figure of the mercury in proportion to its energy. If the metal be oxidated, so as to give a certain tenacity to the superficial film, the radiating currents pursue their course under it; and the supernatant fluid, being thus defended from their action, remains at rest. In this case the only indication of their existence is the protuberance produced by the resultant interior streams.

9. A number of singular appearances are explained by this internal current. In some cases the mercury throws out projections or probosces of inordinate length, which take the direction of the electrified wire, and follow all its motions. The resultant interior current is in this case directed along the axis of the proboscis from its root to its extremity, which thus becomes an indication of a very powerful radiation along its surface in an opposite direction. In others, the mercury flattens throughout its whole extent, and, when this is the case, it is always covered with a thick coat of oxide. In these circumstances the superficial currents tend from the circumference towards the centre of the flattened mass, and the interior stream tends from the centre outwards in all directions,

in a horizontal plane, thus continually urging the circumference farther and farther out, by diminishing the radius of curvature of the vertical section of its edge.

10. That friction against the vessel is the principal cause of the apparent attraction of a globule of mercury to the negative end, may be proved evidently by the substitution of a glass for a Wedgwood-ware basin. In this case the currents are produced as before; but, though equally forcible, the globule shows little or no tendency to move bodily, but if placed on a plate of emeried glass, or on any other rough surface, it will move with great activity; nay, so strong is its tendency to the negative pole, that globules of considerable magnitude may thus be sustained without contact of either wire, on surfaces many degrees inclined to the horizon.

11. It is essential to the production of the motions in question, that the mercury be in actual contact and free communication with the acid, and so situated as to be within the influence of the electric current. It is not necessary, however, that a continuity of the acid should subsist between the positive and negative wires; they will appear in any interrupted circuit of mercury and the liquid medium. The experiment indeed is difficult to try in sulphuric acid, whose capillary attraction for mercury is such that the least drop, applied to any part of a clean surface of that metal, instantly spreads over the whole, but with other conducting media it may readily be made. We have only to drop a little of the liquid to be tried on two different spots of a large clean surface of mercury, and bring the poles in contact with them, taking care not to plunge them in the metal, when the same phænomena will be observed to take place about each pole as

if the whole surface had been covered with the liquid. The motions however are confined to such portions of the mercury as are actually covered, all the rest remaining quite still: the effects too are modified by capillary action.

12. When the circuit is completed in a conducting liquid, in the manner described in the beginning of this paper, the action is most forcible in the direct line joining the poles; its violence diminishing as we recede from this line, though it continues sensible to a great distance either way: and the course pursued by electricity in its passage through conducting media, and its law of distribution within it, may in some degree be traced, by placing globules of mercury in different parts of a liquid; when it will be plainly seen, that it is by no means confined, or nearly so, to the straight line between the poles, or to the surface of the conducting medium, but immediately on quitting the wires diffuses itself through the whole liquid, its density being a maximum in the space directly between them, and diminishing rapidly as we recede from their line of junction.

13. The mechanical action appears (*cæteris paribus*) to be proportional to the absolute quantity of electricity which passes, *dato tempore*, through a filament of the liquid at the point where it is exerted. The magnetic effect is proportional (*cæteris paribus*) to the absolute quantity of electricity in motion present at once, (or at any indivisible instant of time) in a given portion of the conducting wire, or within the sphere of action of the needle, that is, to its density.* To establish or

* In these expressions I have conceived electricity as being transmitted through conductors according to the laws of a gas of high, but variable elasticity through pipes more or less obstructed, a supposition which will represent many of the

refute this distinction, will require experiments which it is easy to imagine, but which I have not yet had an opportunity of making. At first sight, indeed, the phænomena in question present a considerable analogy to the electro-magnetic vortices observed in the fluid metals; but on presenting very powerful magnets to the mercury, while under the circumstances above described, in various positions, I have never been able to perceive any influence exerted by them in accelerating, retarding, or deviating the currents; and moreover, these are incomparably more forcible in proportion to the electric powers used, than the motions produced by the action of magnets.

14. In consequence of this superior energy of action, the phænomena which form the subject of this Paper, furnish a test, perhaps, the most sensible yet known of the developement of feeble Voltaic powers. I constructed a small battery of zinc and copper wires twisted together, each pair being two inches long from the point of junction, and the wires $\frac{1}{30}$ of an inch thick. Ten pairs of these, excited by extremely dilute nitric acid, caused a rapid rotation in mercury, interposed under sulphuric acid between the poles, and a regular advance of

phænomena. The sluggish electricity of a single pair of plates may be compared to air, rendered dense and less elastic by excessive cold, while the active charge of a powerful battery, or the spark of an ordinary electrical machine, is in this view assimilated to air with all its energies exalted, and its density diminished by violent heat. The same quantity in weight may pass through the same conducting pipe in the same time; but in the one case the motion of each molecule will be comparatively much slower, and the actual quantity present at any instant of the discharge (*e. g.* an inch in length) of the conductor, much greater than in the other. I am well aware that this is merely an analogical representation of facts, and of course inaccurate, but it serves to explain the distinction in the text.

globules of that metal towards the negative pole. The rotation continued with considerable force, when the wires were so far withdrawn as to have only their extremities in contact with the liquid in the cells, in which case the surface exposed by each pair to the action of the acid could not exceed $\frac{1}{50}$ of a square inch. Nay, so delicate is this indication, that the electricity developed by bringing the extremities of a thin zinc and copper wire in contact with a glass merely moistened with the above mentioned dilute acid, is abundantly sufficient to cause an immediate and unequivocal rotation in an ounce or two of mercury properly exposed to its action. By this means, indeed, the feeblest electrical excitement may be placed in evidence. I have thus rendered *strikingly* sensible the electricity developed by a mere difference in the state of the surface of two small portions of copper wire from the same coil (one being a little cleaner than the other) not above an inch in length of either being immersed; or that set in motion by a copper and zinc wire held near together and dipped in common pump water, powers which it is not easy to render sensible by other means. For the success of these experiments, however, it is not enough merely to plunge the extremities of the conducting wires under sulphuric acid. The surfaces of contact *here* require to be greatly increased,*

* The efficacy of an increase of surface for transmitting electricity *into* a liquid, is remarkable. By bringing the positive pole in contact with a large surface of mercury, or still better, of an amalgam of mercury and zinc, over which a saline solution is poured, the reduction of the metals of the alkalies and earths at the other pole is operated with a degree of facility hardly to be imagined without trial. In this way the decomposition of ammonia may be effected with thtee pair of single plates of the above dimensions, in very moderate action.

so as to insure the transmission of the whole of the electricity developed. The best way is to immerse them in two considerable pools of mercury under the acid, one on either side of the globule to be set in rotation.

15. Hitherto we have considered only the effect produced when a current of electricity is transmitted over mercury through sulphuric acid. When other conducting liquids and other metallic bodies are used, phænomena of the same kind are produced, but so modified by the nature of the substances employed, the intensity of the electric power, and the manner of conducting the experiments, as to become extremely perplexing; and I must warn the reader who may be inclined to repeat them, that he must expect to find them frequently fail, or even give contrary results from those I shall describe, owing to causes by no means easy to discover. The principal is impurity in the mercury used, and none should be used but what has been carefully distilled, and well washed with dilute nitric acid. It was long before I discovered this necessity; and ignorance of this essential condition engaged me in a series of tedious and disheartening repetitions of every experiment, till I was on the point of relinquishing the subject in despair, encountering contradictory results in operations conducted, as I then supposed, in a manner precisely similar.

16. When mercury, so purified and perfectly clean, is placed in any conducting liquid, and the circuit completed without bringing either pole in contact with the metal, the phænomena vary with the nature of the liquid; but, generally speaking, the effect is the production of currents more or less violent, radiating from the point nearest the negative pole.

In the acids, particularly in the more powerful and concentrated ones, and such as are good conductors of electricity, they are decided and violent. In saline solutions their force is less, in proportion as the electro-positive energy of the base is greater. Thus, in the salts with a basis of potash they are feeble, and often only perceptible by a momentary start of the mercury when the circuit is completed. In those of soda, ammonia, baryta, strontia, and lime, they are more distinct, while in salts of magnesia, alumina, and the metallic oxides, their influence is still more sensible. On the other hand, under solutions of the pure alkalies and alkaline earths, the mercury remains quite quiescent, or at most is only agitated by feeble and irregular motions, depending on causes not now in contemplation.

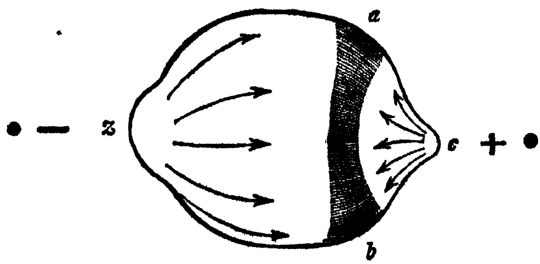
17. In many liquids, and especially in solutions of the nitrates, there is formed not only a current radiating from the negative pole, but also one from the positive, which even has in some cases a preponderance over the other. These co-exist in the mercury; and, in consequence of their action, a zone of equilibrium is formed in the globule, nearer to one or the other pole, as the antagonist current is more or less violent. The best way to render the influence of this counter-current sensible, is to operate on a large quantity of mercury, under dilute solutions, keeping the negative pole at a distance, and the positive very near. In this way there are few liquids which, when the pile is in good action, do not show some signs of a counter-current from the positive pole. The cause of this will be evident, when we come to speak of the action of metallic alloys.

18. If either pole be brought in contact with the mercury, no

currents are observed from the point of contact (at least when the mercury is fresh and the contact perfect) but strong ones are always produced, radiating from the other. If it be the negative pole which is made to touch, it amalgamates with the mercury, which remains bright, and the currents radiating from the positive are visible to the eye, and generally very powerful. On the other hand, if the positive pole be in contact, the oxidation of the metallic surface is usually so rapid as to prevent the currents becoming visible, but a momentary start of the surface from the negative wire, the flattening of the globule, and the protuberances it throws out in pursuit of the oppositely electrified conductor, sufficiently indicate their existence under the crust of oxide. Where this oxidation however does not happen, or is prevented by the addition of a few drops of dilute nitric acid, the currents from the negative wire are equally evident with those from the positive, just mentioned.

19. These however are not the only effects produced by contact with the electrified wires. On breaking the contacts and completing the circuit in the liquid, the mercury is found, for the most part, to have acquired new properties, or lost some of its former ones. A globule of four or five hundred grains of pure mercury being introduced into a solution of sulphate of soda, the circuit was completed in the liquid with neither pole in contact. A current was produced from the negative pole. A momentary contact being made with that wire, and the circuit then completed as before in the liquid, a counter-current was produced from the positive pole, more confined in the sphere of its extent, but apparently more violent in its action than that from the negative. In conse-

quence, the globule acquired the figure here annexed, having a blunt elongation at *z*, the point nearest the negative pole, and a more pointed one at *c*, that next the positive, with a kind of shoulder at *a b*. The film of oxide produced at *z*



was thus swept towards *c*, but never attained beyond the zone *a b*, where it remained stationary and constant in quantity, being absorbed at the side next *c* as fast as it was produced at the other. Another short contact was now made with the negative wire, and, on breaking it, the currents from *c* were found to have increased both in strength and extent, while those from *z* were proportionally enfeebled, the zone of equilibrium *a b* being thus brought nearer to *z*. By another contact prolonged a few seconds, the negative currents were contracted within a very small space around *z*, and by prolonging the contact a little longer, its influence was totally destroyed, and a regular and violent circulation from + to — established throughout the whole globule.

20. But the effects did not stop here. On prolonging the contact a considerable time, the negative current (from *z*) was not only wholly destroyed, but changed into one of a contrary tendency; *i. e.* radiating in all directions *to z*; the particles of the mercury appearing to be attracted to that point with a force equal, or superior, to that with which they were

repelled from *c*. The positive pole being held at some distance, and the negative directly over the surface, any scum or impurity on the mercury was observed to collect directly under it, in a small circular spot, following exactly its motions; and when this was cleared away, the fluid metal was violently thrown up towards the wire in a jet of two or three tenths of an inch in height.

21. The mercury was now brought into contact with the positive wire. Visible oxidation did not commence on its surface for a long while, during which time violent currents still continued to radiate in all directions from the wire and *towards* the point *z* (or in a direction opposite to what they would have taken in untouched mercury). By degrees, however, a counter-radiation commenced opposite to the negative pole, whose sphere was at first very limited, but gradually extended, producing a zone of equilibrium, which advanced rapidly towards the positive wire, and at length attained it. The instant this took place, the oxidation of the mercury commenced at *z*, and speedily extended over the whole surface, forming a thick crust.

22. If the contact of the positive pole was continued long enough, the mercury, on cleansing it from its coat, was found reduced to its former state, as if freshly introduced; but if broken as soon as the crust was fully formed, a radiation from the negative wire was produced, and the crust broken up and swept by it to *c*, where it collected, and was hurried off. But the moment this was done, and the surface of the mercury had become bright throughout, it stopped for an instant, and immediately a violent revulsion took place and a powerful current radiated from *c*, that from *z* being annihilated.

23. These effects, when first observed (not connectedly in regular succession, as here set down, but piece-meal), appeared exceedingly perplexing; but the key to them was soon found. I observed that the effect of a contact of the negative pole was proportionally stronger in producing a positive radiation, as the mercury had been allowed to circulate longer before the contact was made, and, on more close examination, I found that the platina wire terminating the negative conductor of the pile, had got amalgamated with a little mercury, which, during the time the circuit was completed in the liquid, had become alloyed with sodium; and, with the quantity of this metal judged to be present, the effect seemed always to be in proportion. I had no hesitation, therefore, in attributing all the new properties acquired by the mercury to the presence of sodium, and on introducing into a quantity of the pure metal a small quantity of an amalgam of this substance prepared for the purpose, I found my supposition verified; a most violent negative rotation being immediately produced on completing the circuit, without allowing either wire to touch the mercury.

24. The presence of this highly electro-positive metal therefore counteracts the effect of the negative pole, and exalts that of the positive in a degree proportioned to its quantity, till at length it completely overcomes, and even reverses the former effect. As the quantity (in the foregoing experiment) diminished in the alloy by the oxidating action of the positive pole, the mercury, as we have seen, by degrees resumed its original properties. The only effect that may appear obscure, is the revulsion noticed in the direction of the currents when the last portion of oxide disappears. It is, in fact, a pretty com-

plicated effect, but capable of easy explanation. The oxidation takes place over the surface of the metal before the last portions of sodium are removed. This is easily proved. We have only to break the circuit altogether, and the crust of oxide will gradually disappear (unless suffered to go too far), being reduced by the sodium beneath it. Were it then not for the crust of oxide, the currents, as has been seen, would be in a positive direction. But the oxide, acting on the stratum of metallic molecules immediately below it, deprives them of their alloy, which it converts into alkali, leaving a stratum of pure mercury. Now we have seen that in *this*, the rotation, in the circumstances of the experiment, would have a negative direction. We have only then to admit that the peculiar action by which the rotations are caused, is confined to the common surface of the mercury and liquid, to have a perfect idea of the mode in which the whole process is carried on. The stratum of pure mercury on the surface is removed by a negative current agreeably with its natural relations, and immediately succeeded by a stratum of the sodiuretted metal from the interior; this, in its turn, is deprived of its sodium by the oxide in contact with it, and is immediately radiated off like its predecessor, and so on till the whole crust of oxide is exhausted or swept off, when the remaining mercury, still retaining an excess of sodium, and instantly rendered homogeneous, is acted on as an alloy in the way already described.

25. That sodium is actually present in the mercury when it has acquired the property of producing currents from the positive pole, (which for brevity I will hereafter call the positive property) by contact with the negative wire, may be shown by

a very simple and interesting experiment. When the negative wire is detached and the circuit broken, the mercury lies quiet at the bottom of the vessel, with the exception of a slight irregular motion on its surface, and now and then a minute gas bubble disengaged. Now touch it under the liquid with a clean metallic wire of any kind (provided its extremity be not allayed with sodium), and a violent action instantly commences. The mercury rushes on all sides to the wire in a superficial current as if to give out its sodium, while a copious stream of hydrogen is given off from the wire, not merely at the point of contact with the mercury, but wherever it touches the liquid. In a word, the sodium, the wire, and the liquid form a voltaic combination, and the electricity produced by the contact is sufficiently powerful to decompose the aqueous portion of the latter in great abundance. The action lasts for a longer or shorter time accordingly as the mercury is more or less highly charged with the alkaline metal, rarely, however, for more than 10 or 12 seconds, and when over, the mercury is found to have lost its positive property, and to be reduced to its pristine state, (provided the contact be made with copper or platina), which a long immersion in the fluid without such contact would not have entirely effected.

26. If the mercury thus charged with the alkaline base be not entirely covered with the fluid, and the metallic contact be made at the vertex of the globule, out of the liquid, no effect is produced; but if the other end of the metallic wire be bent round and brought to touch the liquid at some distance from the mercury, the violent action above described immediately commences; with this difference, that *now* the surface

of the mercury is radiated in all directions *from* the point of contact to the circumference of the globule, and that the whole of the hydrogen is given off at the other end of the wire where it touches the liquid. A little consideration will suffice, however, to show that both these effects are merely modifications of one and the same. It is not to, or from the *wire* as such, that the superficial particles radiate; they merely follow the direction of the predominant electric currents in their passage *through the liquid*. It is in fact the case of the source of positive electricity, being the mercury itself, instead of its being conveyed to it from a pile at a distance.

27. Having thus distinctly traced the alteration in the mechanical effect by contact with the negative pole, to the amalgamation of the mercury with sodium, the knowledge of this fact led me to investigate more minutely the effects of different metals in their contact and amalgamation with mercury; and the results I have encountered in the course of these enquiries, appear to me so remarkable, that I cannot forbear annexing them, especially as they afford an explanation of almost every anomaly which perplexed me in the commencement of the investigation. In order to render the effects less liable to objection, as well as more distinct and striking, I now used solutions of potash or soda, pretty highly impregnated with the caustic alkali, for the conducting liquid. This has the advantages at once of high conducting power, and of producing no currents whatever in pure mercury, neither pole being placed in contact. Of course, whatever motions arise on the introduction of an extraneous metal must be due entirely to the presence of that metal, and the mercury may

be regarded as merely passive, so far at least as mechanical action is concerned.

28. *Potassium.* A contact of a single second's continuance with the negative pole of a pile of eight pairs, in feeble action under liquid potash, imparted to 100 grains of mercury the property of rotating violently from the positive to the negative pole, the circuit being completed in the liquid alone. The rotation was forcible when this alloy was diluted with 100 grains more of pure mercury, and was still sensible after the addition of another equal quantity. In this latter case, the quantity of potassium present could hardly be estimated at a millionth part of the whole mass.

29. *Sodium.* Under a solution of soda I electrised 100 grains of mercury during 80 seconds with the above mentioned Voltaic power, the mercury being in contact with the negative wire. It was then washed hastily, and introduced under a glass bell into dilute muriatic acid, which disengaged 95 mercury grain measures of pure hydrogen. Consequently, it contained less than $\frac{1}{50}$ of a grain of sodium; and as in such extremely small quantities the production of the alloying metal must go on uniformly, a contact of 1″ would have produced only $\frac{1}{80}$ of the quantity, or $\frac{1}{4000}$ of a grain; that is $\frac{1}{400,000}$ of the whole mass. This being premised, a contact of 1 second in duration was made under similar circumstances, with 100 grains of fresh mercury, which was thus found to have acquired a powerful rotatory property. This was now diluted with 100 grains more of the pure metal, in which, therefore, the sodium was only in the proportion of 1 to 800,000. The rotation was enfeebled, but was still full and distinct. Being again diluted with 100 grains more of mercury, so as to make the propor-

tion of sodium 1 : 1,200,000, there was still a considerable radiation from the positive pole, but not extending over the whole surface. On reducing the proportion of sodium by a third addition of an equal quantity of the pure metal to 1 : 1,600,000, a feeble radiation was still sensible in the same direction.

30. *Ammonium.* A considerable quantity of the amalgam of this singular substance introduced into mercury under a solution of soda *did not communicate to it any power of rotation.* This remarkable result, which goes to separate ammonium by a definite character from the other metallic bases of the alkalies, was again obtained on repeating the experiment. It is possible, indeed, that a complete insolubility of the amalgam in pure mercury may be the cause of this want of action, but the supposition must be allowed to be a very forced one.

31. *Barium.* This metallic body amalgamates with the utmost readiness with a power of eight pairs of plates when the muriate is acted on; a small globule of mercury at the negative wire throwing out beautiful arborescences, and fixing into a highly crystalline, pretty permanent, solid amalgam. A very minute quantity of this introduced into mercury under solution of soda, gives it the positive property. Its efficacy, in reversing the direction of the currents, is strikingly sensible when introduced into a quantity of mercury kept in a state of negative rotation under oxalic acid. The amalgam of mercury and barium added in small quantities to pure mercury, imparts to it the same property as we noticed in the case of sodium, of forming a Voltaic combination with a wire brought in contact with it under a saline solution, and the action so produced is much more lasting.

32. *Strontium, Calcium.* These metals, in my experiments with the feeble powers used, manifested a remarkable indisposition to alloy with mercury. The small quantity of calcium deposited on an amalgamated negative wire obstructed its contact with a larger globule of mercury to such a degree, that no electric communication could be established. Under a solution of strontia, the contact of the negative wire imparted the positive rotatory property sensibly, though very feebly. That this was not merely owing to the low conducting power of the liquid, was proved by introducing a minute quantity of the amalgam of zinc, when the mercury immediately commenced rotating strongly. The influence of *magnesium* is more sensible than that of strontium or calcium, from the greater readiness with which it amalgamates.

33. *Zinc.* When pure mercury is electrified under solutions of potash or soda, with neither pole in contact, in the manner so often alluded to, it shows no signs of rotation, as has already been observed; but, if touched for an instant with the end of a clean zinc wire, or if an atom of the solid amalgam of zinc, the smallest that can be taken up on the end of a needle, be added to it, it instantly rotates violently in a positive direction (or from the positive pole).

34. An alloy of one part zinc to 10,000 of pure mercury rotates with the utmost violence. When this is diluted with ten times its quantity of the latter metal, the force of rotation appears but little impaired. The proportion of mercury was increased to 400,000 : 1, and the rotation, though feeble, was yet complete, pervading the whole of a considerable mass of the alloy; and even when the zinc amounted to no more than a 700,000*th* of the whole, a current radiating to a short dis-

tance from the positive pole was still sensible: when, however, the zinc formed only a millionth part, no difference could be perceived between the alloy and pure mercury.

35. *Lead.* An alloy of 200 parts of mercury and 1 of lead possessed the positive property in perfection. When the proportion of mercury was 667 to 1, the rotation was still produced, but was not full and regular. When increased to 1000, a slight, but sensible current, was perceived to radiate from the positive pole to a short distance; but a proportion of 2000 mercury to 1 lead extinguished every trace of motion.

36. *Tin* acts also in the same way, and with nearly the same energy, as far as I could judge by the eye. It is certainly much inferior to zinc.

37. *Iron* communicates the property in question, though present in such minute quantity as not to be detected by prussiate of potash On the other hand,* *Copper* does not communicate it, though its proportion be increased to such a degree as to give a blue solution in nitric acid, and even to render the mercury quite sluggish.*

38. Of the other metals I have tried, *Antimony* is the only one which appears to exert a perceptible action, and this is so slight (never amounting to more than a mere start, or slight convulsion of the surface at the first impression) that I am inclined to attribute it to impurities in the antimony used, especially as this metal stands very low in the scale of electro-

* The amalgam of iron obtained in one experiment was a white friable solid of a lustre between silver and iron: the mercury being driven off by heat, the iron took fire, and glowed like a live coal till reduced to the state of black oxide, soluble in muriatic acid, having all its characters.

positive energy. Bismuth, silver, and gold, though present in considerable quantities in the mercury, impart to it no power of rotation whatever.

39. This property then of the metals, bears an evident relation to their electro-positive energies. It even affords something like a numerical estimate of them; rude indeed, and liable to a thousand objections, but still not without its value in our present state of complete ignorance on that most interesting of all chemical problems. If it be true, that the whole of chemistry depends on electrical attractions and repulsions, every thing which offers a prospect, however remote, of one day arriving at an exact knowledge of the intensities of these forces, must be regarded as of consequence. It may be objected, that it is only the excess of the electro-positive energy of the alloying metal over that of the mercury, or the alloy over the liquid, that we measure in these experiments, by the quantity of it required to impart a certain appreciable momentum. Yet it is something to have rendered it probable, that this excess in the cases of sodium, zinc, and lead, are in proportions not *very* remote from 1,600,000; 700,000; and 1000; or 1600, 700, and 1. The effect being purely mechanical, even the intensity of the motive forces exerted on a molecule of one of these metals could be determined, did we know the law of its action—but at least, in our ignorance of this, we are sure that it must be incomparably superior to gravity. A mass of mercury an inch in diameter alloyed with $\frac{1}{100,000}$ its weight of zinc, revolved with a motion so rapid as to complete the transfer of particles floating in the liquid in less than a second across its surface. Now, even if we were to take the supposition of a uniform acceleration of the

motion of a molecule from one end to the other of this transfer, the intensity of gravity being taken at unity, that of the force accelerating each particle of the alloy would amount to $\frac{1 \text{ inch}}{16 \text{ feet} \times (1'')^2} = \frac{1}{12 \times 16} = 0.00521$, and each particle of zinc being loaded with 100,000 times its weight of inert matter, the intensity of the force, acting on its molecules, cannot possibly be so little as 521 times their gravity. But it is in all probability immensely greater. So far from being uniformly accelerated along their whole course, the molecules, if narrowly watched, will be evidently seen to move with less and less velocity as they recede from their point of radiation; and it is assuming little to suppose their velocity at a hundredth of an inch from this point double of their mean velocity with which they traverse the diameter. To produce this effect, the force must (if supposed to act uniformly through *this* small space) be increased 100 fold, or to an intensity upwards of 50,000 times that of gravity. Such considerations tend, if I mistake not, greatly to enlarge our views of nature, and to prepare us for the admission of the most extravagant *numerical* conclusions respecting bodies less within the reach of our senses. That such minute proportions of extraneous matter should be found capable of communicating sensible mechanical motions, and properties of a definite character, to the body they are mixed with, is perhaps the most extraordinary fact that has yet appeared in chemistry. When we see energies so intense exerted by the ordinary forms of matter, we may very reasonably ask, what evidence we have for the imponderability of any of those powerful agents to which so large a part of the activity of material bodies seems to be owing?

40. I was anxious to examine whether similar motions would be produced in other metals than mercury and its alloys, when in fusion. The foregoing experiments, indeed, leave little room to doubt their capability to do so; but the nature of the case throws great difficulties in the way of direct experiment. I have been successful hitherto only in the case of the fusible alloy of lead, tin, and bismuth, no mercury being present. This, with a little management, may be preserved tolerably clean of film and air bubbles, when kept in fusion under a boiling solution of sugar, acidulated with phosphoric acid, in which case the same circulation takes place as in the case of mercury, viz. from the negative to the positive pole. When solution of sugar alone however was used, the influence of the tin and lead became sensible, the predominant radiation being from the positive pole; a feeble counter-current being, however, observed from the negative.

41. The contact of the positive pole, in like manner, communicates peculiar properties to mercury, but less strongly marked, and which appear to depend, in part, on the film of oxide formed on its surface, and partly on an absorption of oxygen by the metal itself; a thing rendered not improbable by the analogy of silver and other metals, which when fused in contact with air, absorb oxygen without losing their metallic appearance. The facts I have observed are chiefly these:

42. Equal quantities of mercury were electrified for equal times in two separate capsules, under similar solutions of carbonate of soda, one in contact with the negative wire, and the other with the positive. On mixing them together, the mercury was acted on as if pure, and showed no signs of

containing sodium. Here, the mercury in contact with the positive pole had acquired a virtue capable of counteracting the effect of a considerable impregnation of sodium, which, had it not been counteracted, could not fail to be violent.

43. When mercury is kept in contact with the positive pole, the surface contracts a film of oxide of more or less considerable thickness. Now, break not only the contact, but the circuit. The mercury will be quite still; but the moment it is touched with a clean metallic wire (not electrified), the oxide disappears rapidly at the point of contact, as if absorbed, and the remainder rushes in on all sides to supply its place, producing a system of current in the surface radiating towards the wire. It is not indifferent with what metal the contact is made; potassium, sodium, barium, tin, and zinc, are those which produce the most violent action, the surface brightening instantly with a kind of flash like the *brandishing* of melted silver, tin being in this respect superior to zinc. The effect of iron is pretty considerable, that of copper less so, and of antimony and platina, none at all; neither had phosphorus any effect.

44. The effect, therefore, depends on the oxidability and amalgamating property jointly; and this points out the *modus operandi.* An amalgamation takes place at the point of contact, and this brings the oxidable metal into chemical contact with the oxide immediately around that point, which is instantly reduced. The motion of the surface is, however, doubtless an electric effect, for when mercury, *not* recently electrified is touched, under acids, &c. with metallic wires, the effects are not the same. The contact of copper, for instance, produces an immediate, and even strong radiating

current *from* the point of contact instead of *to* it, and this ceases the moment the contact becomes perfect by amalgamation, and cannot be renewed but by cutting off the amalgamated end, and making a fresh contact.

45. When mercury is electrified in contact with the positive pole under *certain* metallic solutions (nitrate of copper for instance), and the circuit broken, removing both wires, the current continues feebly for some time after the electric power is withdrawn, in the same direction (viz. from the point (z) opposite to the negative pole. By degrees, it grows more forcible, and a film formed during the electrisation is swept along to the point (c) opposite the former position of the positive wire, where it accumulates, leaving at length, the portion of the surface at z quite bright As soon as this happens, the currents increase considerably in strength, and radiate with great violence from the point z. This spontaneous action continues often for a long while. If the negative pole be made to act in succession, opposite to two points z, z', of the mercury, and be then quickly withdrawn and the circuit broken, both these points become centres, from which spontaneous currents radiate simultaneously in all directions. If the negative pole be made to act vertically over a large flat surface, when the circuit is broken, a violent spontaneous radiation emanates from the point immediately below the place where it was situated.

46. If the wires be only withdrawn so as to complete the circuit in the liquid, the film formed during the contact of the positive pole is swept to the point c, opposite that pole; and a violent current is established, radiating from z to c. If this be suffered to continue some time, and the circuit be then

broken, the motion continues as if the electricity still passed; but if the mercury be agitated, so as to break the crust collected at *c*, the regularity of the motion is disturbed: the surface of the mercury is thrown into a kind of fritillation, owing to an immense number of minute and very rapid vortices; and it is not till after some time that a regular and uniform direction of the currents is re-established.

47. These phenomena demonstrate the existence of a system of currents radiating *towards* every molecule of the crust on the surface. In consequence of this, so long as the latter is broken up into small portions and distributed over the whole surface, the currents are irregular and undecided; but as soon as these portions begin to be swept together and collected, they assume a uniform direction, viz. towards that part where, from contact of the vessel or other cause, they meet with no counter currents to oppose them. In what manner the crust acts is however still a little obscure: in all probability it forms a Voltaic combination with the mercury and the liquid.

48. In reasoning upon the facts detailed in this Paper, we have to consider, as probably materially influencing the results, first, the vast difference of conducting power between the metallic bodies set in motion, and the liquid under which they are immersed. This is not unlikely to enter as one of the essential conditions of the phænomenon, especially as it appears to result from all the experiments, that the peculiar action, whatever it be, by which the currents are produced, is exerted only at the common surface of the fluids. I have never been able to produce the least trace of such currents without the presence of a fluid metal. This leads us to conclude that a second essential condition is a perfect immiscibi-

lity of the conducting fluids, so as to render the transition from one to the other quite sudden. Besides these, a third essential condition is to be found in a certain chemical, or electrical relation between them. Under these conditions, it is by no means impossible, that the phænomena may admit of complete explanation from what we already know of the passage of electricity through conductors, and the high attractive and repulsive powers of the positive and negative electricities *inter se.* It is very possible, for instance, that a highly electro-positive body, as potassium, present in the mercury, may have its natural electric state exalted by its vicinity to the positive pole; and, being thus repelled, may take the only course the resistance of the metal on the one hand, and attraction of cohesion on the other, will permit; viz. along the surface, to recede from the positive pole. It *may* even act as a carrier of positive electricity, which *may* adhere to it too strongly to be transmitted through the mercury (which, though a good, is far from a perfect conductor;) and when arrived at the opposite side of the globule, may there, by the influence of the opposite pole, lose its exalted electrical state. This explanation tallies with that of other phænomena which have been attributed to a similar cause; I mean the tendencies observed in the vapours of electro-positive and electro-negative bodies to conductors electrified oppositely, which Mr. BRANDE has described in a Bakerian Lecture formerly read to this Society. Yet it must not be concealed that this explanation is beset with difficulties, and that the mode of action of the less-conducting medium in it is far from clear; it does not even appear why such a medium is at all necessary, unless we conceive it to retard, or otherwise modify the electric current, in its passage through

it, and dispose it thereby to ready combination with the metallic molecules.

49. Another course is doubtless open to us, which is to consider the action which takes place at the common surface of two unequally conducting media, as one, *sui generis*, and to depend on a new power of the electric current of a nature, bearing some analogy to the magnetic action, or possibly resulting from it; but this in the present state of our investigation would be too bold an hypothesis, especially as it is also a very vague one.

50. But whatever conclusions we may form, the phænomena are certainly interesting, and promise to afford abundant matter for future research. Meanwhile, it is not improbable that many phænomena of minute intestine motions usually attributed to capillary attraction, generation of heat, or other causes, may be referable to similar causes. One I cannot forbear to mention, from the striking *external* resemblance of the effect to some of those described in this Paper. I mean the motions described by M. Amici in the sap of the chara, as originating in certain rows of globules disposed in the direction of the stream. The motion of the fluid in the vicinity of these globules has been attributed by M. Amici himself to electricity developed in some unknown manner by them, and is so similar to what takes place when a stream of electricity is made to pass over a row of minute globules of mercury under a conducting medium, that one has difficulty not to presume an analogy in the causes.

J. F. W. HERSCHEL.

Slough, January 6, 1824.

NOTE.

51. Since writing the above, Mr. FARADAY has been so good as to show me a Paper, published by M. SERRULAS, in the Journal de Physique for 1821 (vol. 93,) in which are related one or two of the appearances described in this Lecture, and other very curious ones referable to the same causes (though not apparently regarded by him as being so.) As the phænomena themselves are interesting, and the theory of them adopted by him is (as I shall easily show) insufficient, I shall be pardoned for extracting the whole passage from his Memoir; regretting at the same time not having been able to find a former Paper on the subject, mentioned by him, in which his explanation is given at full length.

52. The phænomena in question relate to the singular gyratory motions assumed by alloys of potassium when floated in small fragments on mercury under water. After noticing those of the alloy of bismuth, which he describes as particularly forcible and lasting, he goes on to say,

53. " Ne seroit-il pas intéressant d'étudier l'action électrique qui se manifeste dans cette circonstance pendant l'oxidation du potassium."—" Elle me semble digne d'attention pour sa liaison avec la décomposition de l'eau dont elle depend uniquement. * * * *

54. " La pellicule légère qui se forme dans ce cas n'est que le bismuth divisé provenant de l'alliage retenant entre ses molecules des bulles d'hydrogène extrêmement fines. Cette pellicule, comme je l'ai dit, est attirée avec une grand promptitude par les substances métalliques mises en contact avec le

mercure sur lequel les fragmens d'alliage sont en mouvement.

55. " J'ai du considérer cette pellicule comme jouissant de l'électricité positive, attendu qu'elle se porte vivement vers l'extrémité negative d'une cuve en activité, et qu'elle est au contraire puissamment repoussée par le pole positif. Si les deux conducteurs touchent seulement l'eau du bain, l'attraction et la repulsion ont lieu dans le sens indiqué. L'effet est encore le même si l'un des fils touche le mercure, et l'autre l'eau. La pellicule se fixe au pole negatif d'où elle est chassée avec force par l'approche du pole opposé. Elle s'écarte, et l'hydrogène de l'eau décomposée se dégage sur ses bords qui dans ce cas font partie du conducteur et le terminent. Si les deux fils plongent dans le mercure il est bien entendu qu'il ne se manifeste plus rien.

56. " Quand, au lieu d'eau simple, le bain de mercure est couvert d'une dissolution peu chargée du chlorure de sodium, le tournoiement des fragmens est plus lent. L'hydrogène produit se trouve engagé et retenu presqu'entièrement par la pellicule du bismuth; l'eau en devient nebuleuse. A l'instant où l'on a plongé dans le bain une tige métallique, on remarque autour de celle-ci un frémissement; les mouvemens cessent et sont arrêtés tant que la tige reste plongée; elle fixe la pellicule dans toute l'étendue du bain; les fragmens d'alliage y sont emprisonnés; mais aussitôt que la tige est retirée, *l'effluve d'hydrogène écarte la pellicule, et les mouvemens recommencent.*

57. " Un fil plongé sur un point quelconque d'un bain ou tournoie l'alliage, même dans un endroit éloigné de ce tournoiement, la partie plongée de ce fil se couvre en peu de

temps d'une multitude des bulles d'hydrogène. Ne pourroit-on pas encore d'après cette observation, *qui prouve que toute la surface du bain est parcourue d'hydrogène*, ne pourroit-on pas trouver dans l'émission rapide et abondante de ce gas la cause de l'électricité, quand on considère que l'air atmosphérique dirigé avec une soufflet sur un carreau de verre donne a ce carreau l'électricité vitrée; ou bien cette effluve d'hydrogène qui pousse vivement sur le mercure les molecules de bismuth non amalgamé, qui les réunit sous forme de pellicule, produit entre les deux métaux un frottement qui développe cette électricité."

58. From these passages it seems natural to collect, that M. Serrulas conceives, 1st, the production, motion, &c. of the pellicle on the surface to originate in the actual mechanical impulse of streams of hydrogenous matter (*effluve d'hydrogene,*) radiated in all directions from the potassium in the moment of its oxidation. That, 2ndly, this bodily radiation of hydrogen is propagated along the surface to any distance. That, 3rdly, the hydrogen disengaged in bubbles from a metallic wire plunged into the mercury is this actual radiant hydrogen, conveyed and collected on its surface from all parts of the mercury. That, 4thly, the friction of the hydrogen so radiated produces the electricity, and not the electricity the hydrogen. And, lastly, that the gyration of the fragments themselves is a consequence of the re-action of the hydrogen they dart out during their oxidation by the water.

59. All these phænomena, however, are much better accounted for on the principles of this Lecture, from a knowledge of the properties conferred on mercury by alloying it with potassium; but, first, it is necessary to premise, that the mere

contact of a metal capable of amalgamating, even for an instant, communicates its peculiar properties, almost in the moment of contact, to the whole mass. The experiments in Art. 33, abundantly prove this; and it may be readily shown also by the following. Let a quantity of mercury be placed in a vessel of muriatic acid; no action takes place; but if touched with a zinc wire it presently becomes covered with bubbles, copiously disengaged from every part of the surface.

60. In the circumstances of M. Serrulas's experiments, it is therefore obvious that his mercury must have been always sensibly impregnated with potassium and the supernatant liquid, a solution of potash; and that it was so, is proved by the effects of the electric current, which agree precisely with those I have stated, as being always produced in such circumstances (Articles 18, 28;) but the cause assigned to these effects by Mr. S. viz. the electro-positive energy *of the pellicle*, is proved not to be the real one by the simple fact, that the violence of the motion is always proportional to the cleanliness of the surface, and is greatest when there is no pellicle at all; besides which, the pellicle *here* consisted of metallic bismuth, a substance incapable of producing any such effect as shown in Art. 38.

61. The gyration of the fragments is produced as follows: a strong Voltaic excitement takes place at the point of contact of two metals so different as mercury and potassium. The mercury becomes strongly positive, and the floating fragments negative. The circuit is completed by the alkaline liquid; and the mercury, being alloyed with a portion of potassium, and being itself the positive pole of the combination, we have here the case of Art. 21; and the result, as stated

by M. SERRULAS, is precisely as in that experiment, the currents radiating from the point of immersion. These once produced, drive before them the fragment in which they originate, in the direction in which it exposes the greatest surface to their action.

62. The attraction of the pellicle to a metallic rod plunged into the mercury, is also a direct consequence of the alloy of potassium present in the mercury, as is also the disengagement of gas from the wire. It is, in fact, precisely the experiment described in Art. 25, and has nothing whatever to do either with the floating fragments, or with any hydrogen they may be discharging at the time, farther than that their contact serves to furnish potassium to the mercury.

63. It is needless, therefore, to push this examination farther, as all the phænomena observed by Mr. S. are only particular cases of those I have described. With regard to the radiant hydrogen producing currents by its impulse, I would ask how it happens that currents are produced (when the positive pole is placed in contact), while a thick and tough coat of oxide covers the whole surface; and, one would think, must effectually defend it from the action of the hydrogen. Yet we have seen, in Art. 18, that the currents continue their course under this crust; and it will hardly be contended, that the hydrogen finds a passage between the oxide and the metal.

J. F. W. H.

London, Jan. 13, 1824.

IX. *Experiments and observations on the developement of magnetical properties in steel and iron by percussion:—Part II.*

By WILLIAM SCORESBY, *Jun. F. R. S. E. &c. Communicated by Sir* HUMPHRY DAVY, *Bart. Pres. R. S.*

Read January 29, 1824.

HAVING had the honour of laying before the Royal Society a Paper on the "Developement of Magnetical Properties in Steel and Iron by percussion,"* I beg permission to add to that Communication an account of other experiments; in which much higher degrees of magnetic energy were obtained by percussion, in the employment of new combinations of rods of iron not previously magnetic.

It was shown in the former paper, that the extraordinary developement of magnetism, by this process, arose from the use of a large bar of iron, or soft steel, first rendered magnetic by hammering, in the position of the dipping needle, or in the direction of the magnetic force; for, on applying a similar quantity of percussion to the same bar whilst held on a mass of brass or stone, or even on a bar of iron laid in the plane of the magnetic equator, the polarity elicited did not exceed the twenty-ninth part of that obtained by the use of the vertical rod of iron.

In the subsequent experiments I had two principal objects

* Philosophical Transactions for 1822, p. 241.

in view. First, to try, by a new combination of auxiliary rods of iron, to attain still higher degrees of magnetic energy: and, Secondly, to endeavour to ascertain on what circumstances, as to the magnitude of the rods of iron, and the quality, size, and temper of the steel wires, the highest success of the experiment depends.

In the experiments formerly detailed, a single rod of iron was used, and the steel bars or wires were hammered upon it, whilst both were held in a vertical position; in which case the magnetism of the iron, after hammering, was employed in aid of the power of percussion for the developement of the magnetism of the steel bars. But the magnetism of the iron rods was communicated only to the lower, or *north* end of the steel bars, the polarity of the upper, or *south* end, being merely consequential. Hence it appeared probable, that, were the steel bars or wires placed between two rods of iron, and thus subjected, either directly, or indirectly, through the medium of the upper rod, to percussion, they would derive the advantage of the magnetism of both rods of iron acting consistently, and at the same time, upon their northern and southern poles, so as, I apprehended, by the use of equal rods of iron, to *double* the quantity of attraction formerly developed in the steel.

As, however, two long rods of iron were not so easily managed as a long one for below and a shorter for above, I prepared my rods on this plan, though with the expectation of sacrificing a proportion of power.

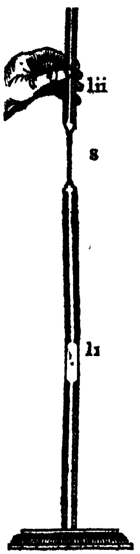
The rods I ordered for the experiment were of the respective lengths of three feet and one foot, and an inch in

diameter. The weight of the former was 8lbs. and of the latter $2\frac{2}{3}$lbs. The end of the larger rod (I i*), designed to be kept upward, was made conical, with the view of concentrating its magnetic force; but this was truncated at the diameter of a quarter of an inch, and a shallow hole drilled in the centre for steadying the steel wires when hammered, the lower ends of which wires were rounded into a blunt point, so as to fit the depression in the top of the iron rod. The lower end of the shorter rod (I ii), was constructed in a similar manner.

As I could not hammer this conical extremity of the large iron rod without destroying its form and face, I made use of another iron rod (I iii), corresponding in size and weight with the rod (I ii), but having its lower extremity hollowed like a cup, to fit upon the conical termination of the larger rod (I i.) The larger rod was generally hammered before each experiment through the medium of this shorter rod, (I iii), which not only served to preserve the other rod from injury, but at the same time tended to augment its magnetism.

In experiments with this apparatus, then, after eliciting some magnetic energy in the rod I i, by hammering it in the way just described, the steel wire intended for receiving the magnetism was placed between the two rods, with the conical

* To prevent circumlocution, I have distinguished the rods and wires used in the following experiments by the initial letters descriptive of the substance of which they were composed, connected with a number to distinguish the different bars and wires of the same quality. Thus I, signifies an iron rod or bar; and S, a steel bar or wire; S *u*, steel untempered, or in the state in which the wire was drawn; and S *t*, steel tempered, or softened by heating to redness, and slowly cooled.



terminations [I i and I ii], the whole three substances forming a continuous straight line, in a vertical position, as in the annexed figure. The upper end of the upper rod (I ii), being now beaten with a small hammer,* acquired magnetical properties, which were communicated to the steel wire, whilst the lower rod receiving some influence from the percussion, performed a similar office. This kind of experiment I have, in the subsequent details, denominated the *compound process;* whilst the hammering of a wire upon the principal rod (I i), only, without the use of the second rod, is called the *simple process.*

To give the experiments the best chance, I made use entirely of steel wire, such as is employed by watch-makers, which I found, from several comparisons and experiments, that I did not think it necessary to detail here, had a much higher capacity for magnetism thus developed, than any other steel that I tried: and for obtaining the best effect, in the trial of the lifting power produced in the steel wires by percussion, I procured a series of nails of different weights, made of good iron, with flat heads, so as to stand with their points upward, in which position they are the most readily lifted; and after blunting their points, I gave them some degree of polish for improving the contact.

* The hammer employed in all the subsequent experiments weighed eleven ounces, inclusive of the shaft. Some trials were made with a larger hammer; but its tendency to bend the wires was so great, as more than to counterbalance the advantage it gave of a more speedy developement of magnetic energy. This larger hammer, however, (weighing twenty-five ounces), was generally used for beating the iron rods before the different experiments, when it was of decided advantage.

These nails were of the following weights in grains :— $1\frac{1}{2}$, 4, $5\frac{3}{4}$, $7\frac{1}{2}$, 18, 37, 73, 88, 112, 186, 246, 326, 389, and 482.

In trials of the lifting power of the steel wires, in the subsequent experiments, the weight of the largest nail that could be lifted is set down.* In some instances a small weight was added to the nail, when the difference between this and the next in the series was considerable ; in this case, the lifting power is stated at the weight of the nail, *plus* the number of grains of the appended weight.

FIRST SERIES,

for determining the superiority of the Compound process over the Simple process, when employed for the developement of magnetism in steel wires by percussion ; and for the trial of the general effect of Temper on the magnetic attraction elicited.

The apparatus consisted of the iron rods I i, I ii, and I iii, together with various steel wires.

EXPERIMENT No. I.

[July 1.]

Steel wire S u i, [namely, steel wire untempered]. Length, 5 inches ; diameter, $\frac{1}{8}$th of an inch ; weight, 167 grains.

This wire was hammered, for a length of time, on the *simple process*, and only lifted 36 grains.

* I am perfectly aware that the lifting power of a magnet is by no means a certain measure of its magnetic force, and that the comparative lifting powers of magnets of various sizes do not afford an exact measure of their relative degrees of magnetic energy ; but this mode of trial was employed both on account of the impossibility of making accurate observations on magnetic intensities at sea, where

The hammering being continued for some time longer, by the *compound process*, the lifting power was sensibly augmented; but it yet refused 73 grains.

No. II.

The same steel wire (S t i) softened by heating to redness.

a. By simple process.

After 5 blows this wire now lifted 73 grains.
4 more blows - - - 186.

The wire being now bent was straightened, by which it lost a great part of its magnetism; it was therefore raised to a lifting power of 186 grains, as before, by a number of very slight blows.

10 gentle blows were now struck, but its lifting power was still - - - - 186 grains.

20 more blows increased its attractive force very sensibly; but it still refused the nail of 246 grains.

These last 30 blows having been productive of very little effect, the power was considered as being sufficiently near the maximum by this process, to serve as a comparison with the compound process.

b. By Compound process.

20 slight blows on the upper iron rod, I ii, now increased the lifting power of the wire to 246 grains.
And 20 more slight blows raised its power to 326.
But 60 more slight blows produced no higher effect.

these experiments were made, and also on account of this being the most palpable and striking test of the high force of attraction attained by the process.

No. III.

[July 2.]

Same wire as in the last experiment [S *t* i].

a. Simple process After repeatedly hammering this wire upon the bar I i, it now lifted 246 grains; but its power could not be farther increased.

b. Compound process. The lifting power, by often hammering, was now raised to $326 + 19 = 345$ grains.

No. IV.

[July 10.]

Wire S *t* i, *the same as in the last experiment.*

The compound process was, on this occasion, continued for a length of time, the bars I i and I iii being repeatedly hammered together, but no additional lifting power could be obtained.

No. V.

[July 10.]

Larger wire, S *u* i i; *length*, 12 *inches*; *diameter*, $\frac{1}{6}$*th of an inch.*

This wire being hammered in its untempered state, with 20 or 30 smart blows, by the simple process, only lifted $7\frac{1}{2}$ grains.

No. VI.

The same wire as in the last experiment, tempered by heating to redness. [S *t* i i.]

a. Simple process.

By 2 smart blows, with a small hammer, it now lifted - - - - - 37 grains.

4 more very strong blows - -	73 grains.
10 more - - - - -	186
8 more - - - - - -	246
6 more [wire began to bend] - -	326
4 more - - - - - -	326

No. VII.

The wire last used (S *t* ii), *not being regularly softened, was again put through the fire, and all its magnetism destroyed.*

a. Simple process.

By 2 strong blows only, it was now made to lift	186 grains.
4 more - - - - - -	246
30 lighter blows - - - -	326
20 more [began to bend] - - -	326

b. Compound process.

After 20 blows on I ii, (the magnetism before given not being destroyed, though much weakened by straightening it) it lifted 326 grains much more freely than before, but refused the next larger nail of 389 grains.

No. VIII.

A piece of the same wire as that used in the last experiment, 2½ *inches in length.* [S *t* iii.]

a. Simple process.

After being repeatedly hammered by this process, its lifting power was raised to 56 grains.

Hammered several times, at intervals, for a minute or two together, without increasing its power to lift 88 grains.

b. Compound process.

After 12 smart blows, under this process, the lifting power was augmented to 186 grains.

SECOND SERIES,

for ascertaining more exactly the comparative effects of a difference of temper on wires of the same quality and dimensions, and of the relative advantages of the different processes for the developement of magnetism by percussion in equal wires.

The apparatus consisted of the three iron rods (I i, I ii, and I iii,) with five new wires, from the same piece, of equal sizes, each of them being five inches in length, and 145 grains in weight; namely, S *u* iv, S *t* iv, S *t* v, S *t* vi, and S *t* vii.

Experiment No. IX.

New steel wire, S *u* iv *(untempered); not in the least degree magnetic.*

a. Simple process.

5 smart blows struck on this wire whilst held on bar I i, (which bar had previously been struck 20 hard blows with the large hammer, through the medium of the bar I iii) occasioned a lifting power of 37 grains.

b. Compound process.

[Magnetism of the wire destroyed.]

5 smart blows on this process, (each of the bars I i and I ii having been previously hammered whilst held vertically in the hand) produced a lifting power of 37 + 18 = 55 grains, and very nearly 73 grains.

No. X.

New Steel wire S *t* iv *(softened), not at all magnetic.*

5 hard blows struck on this wire whilst held on the equator, or middle of the bar I i, (this bar lying horizontally instead of standing vertically as in the other experiments) produced only a lifting power of 4 grains

No. XI.

Similar wire to the last, S *t* v, *not in the least magnetic.*

5 blows struck on this wire, whilst held upon I i, both the bar and wire being in a vertical position, and both being freed from magnetism immediately before the experiment excepting the magnetism of position, occasioned a lifting power of 37 grains, and that with difficulty.

No. XII.

A wire similar to the last in every respect, S *t* vi; *not in the least degree magnetic.*

a. Simple process.

5 blows were struck on this wire, held in the same position as in the last experiment on I i, the bar I i having been previously struck 20 hard blows with the large hammer, through the medium of I iii, by which treatment a lifting power was given to the wire of 112 grains.

The magnetism of the wire being destroyed again, the same experiment was repeated, and somewhat harder blows struck, by which, with 5 blows, a lifting power of 186 grains was obtained.

No. XIII.

Another wire exactly similar to the last in size, quality, and temper.

b. Compound process.

5 blows on this process, through the medium of I ii, (both the bars I i and I ii having been previously hammered to the same extent as before) produced a lifting power of 246 + 19 = 265 grains.

THIRD SERIES,

for determining the effect of larger iron bars.

In place of the rod I i, *a large bar of iron* (I iv) *was substituted. Its length was* 8 *feet, and its diameter* $1\frac{3}{4}$ *inches. The rest of the apparatus (rods* I ii *and* I iii*) was the same as before.*

No. XIV.

[July 14, &c.]

Wire S*t* i *being* 5 *inches in length,* $\frac{1}{8}$*th in diameter, and* 164 *grains in weight,* which, with the former apparatus, was made to lift* 345 *grains.* [*See Experiments* II. III. *and* IV.

a. Simple process.

Though the magnetism of this wire was nearly destroyed before this experiment, a few smart blows on I iv, gave it a ready lifting power of 326 grains.

* The weight of this wire was originally 167 grains; but it was reduced 3 grains by occasionally filing the end.

The experiment being repeated the next day, (July 15), when the bar, I iv, had become more magnetic by use, the wire readily lifted - - - - 389 grains.
A few more blows raised its power to - 482
Additional hammering - - 482 + 19 = 501
Experiment repeated, July 16, 482 + 51 = 533

b. Compound process.

After a few smart blows the lifting power of this wire was now raised to - - - 482 + 103 = 585.
Process continued by a number of blows 482 + 122 = 604.
After a variety of repetitions of this process, the large bar, I iv, having become strongly magnetic, the lifting power of the wire was augmented to 482 + 187 = 669 grains, being above *four* times its own weight.

No. XV.

[July 14 to 18]

A new untempered wire (S *u* iii), *from the same piece as* S *t* i, 4½ *inches in length; weight,* 150 *grains.*

a. Simple process.

After a few smart blows, this wire lifted - 246 grains.
On repeated hammerings, its lifting power was augmented to - - - - 326
The process repeated the next day, - - 389
Again repeated on another day - - 482

b. Compound process.

On the first application of this process, by a few hard blows, there was no accession of power, the lifting power continuing at - - - - - 482 grains.

But on repeating the process several times on the following day, the power rose to 482 + 120 = 602.

This process was again tried some days afterwards, and applied with considerable labour, the hammering being continued for half an hour together, sometimes by one process and sometimes by the other. At the commencement of the operations it was found to have retained of its former energy a lifting power of 389 grains. This was considerably augmented, but it never reached quite so high as before.

No. XVI.

Wire S t ii, 12 inches in length and one-sixth of an inch in diameter; being the same that was used in experiments V. VI. *and* VII. *when the maximum lifting power obtained was between* 326 *and* 389 *grains.*

a. b. Both processes alternately.

After a little hammering this wire now lifted 389 grains.

But after many repetitions on the same day [July 15] it refused 482 grains, though its former power was somewhat increased.

a. Simple process.

Some days after the preceding trial, when the great bar, I iv. had become strongly magnetic (after the conclusion of experiment No. XIV), this long wire was again subjected to experiment, when its lifting power was increased to 482 + 180 = 662 grains.

A piece of this wire, four inches in length, was now cut off, and the larger part was thrice hammered by the simple process, when its lifting power was 482 + 90 = 572, being a loss of 90 grains.

GENERAL REMARKS.

The results of the foregoing experiments were, in general, satisfactory, though there were some trifling anomalies, which will be noticed in their order. The chief points I had in view were tolerably well determined; but the investigation is so far from being complete, that what has been done, as is often the case in such researches, opens a much wider field of enquiry than I at first contemplated. I proceed, however, to state the chief results obtained, consisting of such deductions as I trust the experiments will be considered legitimately to warrant.

1. One principal object of enquiry in these experiments was, to prove the effect of a combination of rods of iron, on a plan previously arranged, for augmenting the magnetic power by percussion. The result was, in the main, perfectly agreeable to my expectations. For, by the employment of such a combination, which I have denominated the *compound process*, the magnetism developed was always more or less increased, but the proportion of augmentation was by no means regular, nor is the law by which it is governed very obvious. In experiment No. II, the maximum effect of the simple process was an attractive force capable of lifting between 186 and 246 grains; whilst the compound process readily augmented the lifting power to 326 grains. And on repeating the trial, (Experiment III.) on the simple process, the wire obtained a lifting power of 246 grains, which the compound process increased to 345. But the advantage was the most obvious in experiment No. VIII, when a very short piece of wire was used. In this case, whilst the simple pro-

cess only occasioned an attractive power capable of lifting between 56 and 88 grains, though the wire was repeatedly hammered, the compound process augmented the lifting power, by 12 blows, only, up to 186 grains, indicating three times the former magnetic energy.

But, on the other hand, in experiment No. VII, the application of the compound process to a very long wire, was productive of very little advantage. Nor was the compound process of so evident an advantage in the third series of experiments: in this case, however, the failure of effect was probably occasioned by the very small size of the upper rod, I ii, in comparison of the magnitude and mass of the lower.

2. Another object of enquiry in these investigations was the effect or relation of *temper*, in connection with the degree of magnetic energy developed. The result with the first apparatus, was conformable to the law of magnetics in general; namely, that the *softer* the temper the more susceptible the steel becomes of the magnetic condition. By a comparison of experiments No. I. with II., V. with VI., and IX. with XII. and XIII., the advantage of softening the wire is very obvious. In the first instance, a steel wire in the state in which it was drawn could only be made to lift between 36 and 73 grains; but the same wire, on being softened, readily lifted 186 grains, after nine blows on the simple process, and after 80 blows by both processes, its lifting power was augmented to 326 grains, being nearly twice its own weight, and at least five times the power it acquired in its untempered state. But this difference of susceptibility for receiving the magnetic energy, is rendered still more obvious by a comparison of the second set of experiments (No. V. and VI.) A long wire, untempered, being struck 20 or 30

smart blows by the simple process, only acquired a lifting power of between 7½ and 18 grains; whilst the same wire, after being softened and subjected to a similar treatment, lifted 326 grains. And in the third set of experiments (IX, XII, and XIII,) the result was analogous. The untempered wire, by 5 blows on the simple process, lifted only 37 grains, and by the same number of blows on the compound process, 73; whilst a similar wire tempered lifted, after exactly the same treatment, 186 grains by the simple process, and 265 by the compound process.

The same result was indeed always obtained in various other experiments, not included in the preceding details, yet there is one in the third series [No. XV.] which is apparently at variance with this conclusion. The difference of effect, however, (the untempered wire in this instance having received an equal power to that of the softened wire of a similar kind), was evidently owing to the employment of a very powerful apparatus, the large bar of which had become highly magnetic from long continued use in these experiments. Hence a greater action upon bars of harder temper was to be expected, conformable to what occurs with the use of a powerful apparatus in the ordinary modes of giving magnetism to steel.

The facility with which magnetism may be developed in softened wires by this process is very striking. The first five blows, in experiment No. II, by the simple process, produced a lifting power of 73 grains, nearly one-half the weight of the wire;—the first two blows, in experiment No. VII, also by the simple process, produced, in a long wire whose magnetism had been totally destroyed by heating to redness, a lifting power of 186 grains; and the first 5 blows, in ex-

periment No. XIII, on a new wire by the compound process, occasioned in this wire, of only 145 grains weight, a lifting power of 265 grains.

3. The increased magnetic energy developed by the use of larger bars of iron (a fact indeed which it was reasonable to expect) is quite satisfactory in all the comparative experiments. The long wire S *t* ii, by the *small* apparatus was made to lift between 326 and 389 grains, being the highest effect produced on it [Experiment No. VII.]; but on applying it to the *large* apparatus, its lifting power was at length augmented to 662 grains. [Experiment XVI.] And by the small apparatus, the highest power that could be given to the wire S *t* i, was a capability of lifting a weight of 345 grains [Experiments II, III, IV,]; but on using the large apparatus, the same wire [Experiment XIV.] was eventually made to lift 669 grains, being above *four* times its own weight. This was the highest effect produced. The advantage, however, of the large apparatus, was the most striking in the case of untempered wires. An untempered wire, S *u* i, had its lifting power, by the first apparatus, with difficulty raised to nearly 70 grains, [Experiment I.] whilst a similar, but somewhat shorter wire, S *u* iii, was, by the use of the large apparatus, readily made to lift 246 grains; and by continuing the process on several different days, its power was at length increased to 602 grains. [Experiment XV.]

4. It would appear from experiments IV. and XV, that in the use of any one apparatus there is a limit to the power which it is capable of developing; whereas, from the great augmentation of effect obtained by the use of a larger apparatus, it becomes probable, that were iron rods sufficiently

large employed, there would be no limit to the attractive force developed in the steel wires, until they were magnetised to saturation. The quantity of effect produced by an iron bar seems to be in some certain proportion to the amount of its own magnetic energy, as indicated by its action on a compass needle, but not in the proportion of its lifting power.

5. It is a well known fact in magnetics, that the capacity of steel for magnetism is increased by time—by repeated renewals of the magnetising process at intervals—and by keeping the magnet under constraint, either by the contact of other magnetic substances, or by the use of conductors between the opposite poles. Hence I expected, that a wire magnetised by percussion to a maximum for the time, might have its power subsequently increased, day after day, in consequence of its capacity being increased by a repetition of the process. To a certain extent this was the case; but when the iron bars had acquired their maximum energy, and the wires were then hammered until there was a decided suspension of increase of energy, no future repetition of the process, however laboriously conducted, gave me any additional power. [Experiments No. IV. and XV.]

On the first view of the subject, I was at a loss to account for the suspension of the augmentation, when I had calculated on an increased capacity for magnetic energy, with the continuation of the process, agreeable to the known laws of magnetics; but I eventually perceived that the analogy, with the ordinary communication of magnetism, was not complete. It was evident that the suspension of augmentation did not arise from the want of capacity in the *steel* for magnetism, the wires not being near a state of saturation; but it arose from the

incapability of the *iron rods* attaining, or retaining, more magnetic energy. The maximum, in their conditions, it would appear, determines the maximum in the condition of the steel. But I apprehend, had I used very large rods, capable of communicating magnetism to the wires *above* the point of their saturation, then the usual analogy would have been complete, and I should, progressively, have attained higher and higher degrees of polarity.

As the phænomenon of the progressive elevation of the point of saturation in magnets became a matter of some thought and consideration with me, I may be excused, perhaps, for including here the substance of my reflections on this subject, and especially as some change, similar to what takes place in magnets having their saturated point raised by renewals of the magnetising operation, &c., also takes place in the disposition of steel bars or wires for polarity, by percussion. For, as a magnet having its poles reversed, will not, at first, by the same apparatus and process as that from which it originally derived its power, acquire the same intensity as before; so, in wires magnetised by percussion, with the same end always held downward, the inversion of the poles will not be accompanied by equal power as before. Respecting this phænomenon, I may suggest the following explanation.

The natural condition of iron is without polarity. It acquires polarity by certain modes of treatment, or juxta-position with magnetic substances; until which it evinces no attractive property. In this state its magnetic properties are neutral, and there is no tendency in the iron to develope polarity. But if it be rendered magnetic, no matter by what

cause, as it is now in a state of constraint or violence, there is a tendency to return to the state of neutrality. In soft iron, the return to its former condition, is almost instantaneous on the removal of the disturbing cause; but in steel, the restoration to its former state is resisted by a force proportionate to the hardness of its temper. Soft steel readily acquires polarity, but it is evanescent; and hard steel receives polarity with difficulty, but it is very permanent. Though in all magnetised substances there is a force always acting towards the restoration of the condition of neutrality, yet, when the magnetism has been highly developed, it produces a permanent change in the point where the force ceases to act. This point we may call the state of *quiescence*, being the state of a magnetic substance, when the different magnetisms, though not perfectly neutralised, are yet, as it were, balanced. In the natural condition of a substance, capable of some permanency of magnetism, the neutral and quiescent states are coincident, but after every magnetising operation, if the denomination of the poles be always preserved the same, they are farther and farther removed; hence arises an increasing capacity for magnetic energy. If, for the sake of illustration, a piece of steel somewhat soft be magnetised: on the removal of the cause (provided no artificial means be used to keep it up), the polarity will gradually diminish until it comes to the quiescent condition, or the state where the resistance natural to the metal has no returning force to oppose. Suppose, in this state, it yet has polarity capable of lifting the weight of an ounce of iron, and that in its highest or saturated state of magnetism it lifted 20 ounces. On being again magnetised by the same means and process, it will now have increased

its capacity to near 21 ounces. By thus continuing the process, and using means to retain the magnetism developed, the quiescent point may perhaps be raised as high as the original state of saturation; that is, to a lifting power of 20 ounces, when its saturated lifting power will approach to 40 ounces. But to effect this, it will be necessary to continue the poles always the same; for on every inversion of the poles, the point of quiescence will return somewhat towards the point of neutrality, and the magnetic energy will be proportionably diminished. On the first change of the poles, the magnetic energy, in a state of saturation, will be diminished by about the difference of the points of quiescence and neutrality, or nearly as much as in experiments with the same poles always preserved, the magnetic energy is augmented. Thus, when in the above example, the point of quiescence has a lifting power of 5 ounces, capable, when magnetised to saturation, of being raised to 25 ounces, on inversion of the poles it will only have a capacity for 15 ounces. These, most probably, are not the exact powers, for it is likely that the augmentation of force, which takes place on repeatedly magnetising at certain intervals beyond the point of quiescence, may diminish in a geometrical ratio, as the point of quiescence recedes from the point of neutrality; otherwise there would be no limit whatever to the power of a magnet. The law, however, not having been determined, I have used in the above examples an arithmetical ratio, merely for the sake of illustration.

This doctrine is the same when applied to the developement of magnetism by percussion, as by any other process, and easily explains why, on the first trial, especially with a

weak apparatus, or with bars not perfectly softened, such great effects should not be produced as by numerous repetitions of the same process at different times. The proof as to the cause being the removal of the point of quiescence from that of neutrality, is, I think, quite conclusive. For in an experiment, (one among many to the same effect), made with the wire S *t* i, I found that when 5 blows with a hammer, on the compound process, with the north end of the wire downward (commencing when the attractive force was almost totally destroyed), gave it a lifting power of 326 grains; the same nnmber of blows, with the south end down, (also commencing when the attractive force was very nearly neutralised), gave it only a lifting power of 2 grains. In short, I have raised the point of quiescence so high above the point of neutrality, that a long and severe hammering, south end of the wire downward, only destroyed the original attractive force, but did not produce inversion of poles.

6. The foundation of this process for the developement of magnetism in steel, is the employment of such magnetism of large rods of iron as can be derived from juxta-position of the great magnet, the earth. And the high effects produced seem to depend on the disposition which percussion gives to the ferruginous particles for assuming that condition to which we give the name of *magnetic*. The iron rod employed, as soon as placed in a vertical position, or in the direction of the dipping needle, acquires polarity from the earth; but the natural resistance of its particles to the state of constraint which it must be in when magnetic, prevents its receiving the full power that the earth is disposed to communicate. Percussion, by producing a vibration among the particles of

the metal, seems to overcome this resistance; and, at the same time, disposes the iron for the retention of the magnetism acquired. Now, on placing a wire or bar of soft steel upon this rod of iron, its magnetic virtue occasions some developement of the same power in the steel, which is increased by the tendency that percussion has to aid its developement in the steel, the same as it previously elicited in the iron. By continuing this treatment, the polarity of the steel is progressively augmented until it has acquired a maximum, depending, not so much on its own capacity for magnetism, as on the capability of the iron to develope it. But this condition, it has been shown, is only a maximum so long as the same iron bar or bars are used, and so long as their magnetic energy obtains no augmentation; for if by any means their polarity be increased, the attractive force of the steel wires will rise in proportion. From the whole phænomena viewed in connection, it seems, that the simple general fact is this:—that percussion applied to magnetisable substances in contact with one another, disposes them to an *equality of condition.*

If this view of the subject be correct, we have a satisfactory explanation of some of the phænomena, which were otherwise obscure. We see why large bars, or wires of steel, (though they acquire a greater quantity of magnetic energy, as shown by their higher action on a compass needle, than small ones) do not attain any higher lifting powers than much smaller wires. [Compare Experiments III. with VII., and XIV. with XVI.] And, above all, we have an explanation of the apparently contradictory propositions—that percussion diminishes, and has a tendency to destroy the energy of

ignets; and that percussion has a tendency to develope polarity in iron and steel not previously magnetic. Both these propositions are undoubtedly true; but they are under different conditions. Percussion, whilst it diminishes the energy of a magnet when held alone, or even upon an iron bar, would probably augment its energy if it were placed with consistent poles in contact with a more powerful magnet, which is the condition of wires magnetised by percussion; because, if they be hammered upon a substance not magnetic, little or no energy is developed. [See Experiment No. X.] These different effects would appear to be the necessary result of a tendency to equality of condition—a tendency which is precisely similar in respect to bodies of unequal temperatures when placed in contact, or even in juxta-position. For as a hot body placed near, or among, cold substances of greater mass and capacity than itself, has its temperature brought down nearly to the state of the colder substances, whilst a cold body has its temperature raised by contact with hot substances; in like manner, a strong magnet hammered upon an iron rod, has its energy brought down towards the condition of the iron; and a bar not magnetic, or slightly so, hammered upon the same rod, has polarity developed in it, and raised progressively up to the condition of the iron.

As I apprehend it would be in vain to attempt to produce such strong magnetic effects, as we derive from percussion, by the simple use of the same bars and apparatus in any of the known modes of *touching*, it becomes probable that this process might be applied, in connection with other modes of magnetising, for giving increased power to magnets. The

extraordinary power of KNIGHT's magnets are well known; though the secret of their manufacture died with him, and no effect at all comparable, has, I believe, been since produced. Is it not probable that he may have employed a process of this description?*

* I am indebted for this thought to a suggestion of Sir H. DAVY's, in a conversation on these experiments.

W. S. *Jun*r.

On board the ship Baffin, off Iceland,
August 27, 1823.

X. *On Semi-decussation of the Optic Nerves.* By WILLIAM HYDE WOLLASTON, *M. D. V. P. R. S.*

Read February 19, 1824.

WHETHER we consider the astonishing subtlety of that medium, which renders visible to us objects existing at the most immeasurable distances from us, or that delicately constituted organ which, by its general structure, collects the rays of light, and by a nice adaptation of its parts concentrates their force on the sentient fibres of the retina, expanded over its inner surface, we can feel no surprise that such great talents should have been devoted to investigate the curious properties of the one, or that the structure of the other should have been examined with so much assiduity.

The keenness of inquiry manifested by the cultivators of anatomy in observing the most minute parts that have escaped the notice of their predecessors, shows that any addition to the common stock of our information on this subject will be gratifying to a certain portion of the members of this Society, and probably not uninteresting to the Society at large.

It is not my object, in the present paper, to examine either the *first* effect of the cornea in rendering the rays of light convergent, or the power of the crystalline lens in *finally* bringing them to a focus on the retina. It is not my intention to investigate whether the adaptation of the eye to different distances is effected by alteration of the *form* of the lens from

its own muscular structure, or by alteration of its *place*, from the agency of other muscles. Nor do I mean to consider either the *involuntary* motions of the iris dependent on the quantity of light present, or that *voluntary* contraction of it by which we adapt the aperture of the pupil for distinct vision at different distances, limiting thereby, what in optics is termed the spherical aberration of the lens.

The subject of my inquiry relates solely to the course by which impressions from images perfectly formed are conveyed to the sensorium, and to that structure and distribution of the optic nerves on which the communication of these impressions depends.

Without pretending to detect by manual dexterity as an anatomist, the very delicate conformation of the nerves of vision, I have been led, by the casual observation of a few instances of diseased vision, to draw some inferences respecting the texture of that part which has been called the decussation of the optic nerves, upon which I feel myself warranted to speak with some confidence.

It is well known that in the human brain these nerves, after passing forwards to a short distance from their origin in the thalami nervorum opticorum, unite together, and are, to appearance, completely incorporated; and that from this point of union proceed two nerves, one to the right, the other to the left eye.

The term decussation was applied to this united portion, under the supposition that, though the fibres do intermix, they still continue onward in their original direction, and that those from the right side cross over wholly to supply

the left eye, while the right eye is supplied entirely from fibres arising from the left thalamus.

In this opinion, anatomists have felt themselves confirmed by the result of their examination of other animals, and especially that of several species of fish, in which it is distinctly seen that the nerves do actually cross each other as a pair of separate cords, lying in contact at their crossing, but without any intermixture of their fibres.

In these cases it is most indisputably true, that the eye upon the right side of the animal does receive its optic nerve from the left side of the brain, while that of the left eye comes from the right side; but it is not a just inference to suppose the same continuity preserved in other animals, where such complete separation of the entire nerves is not found.

On the contrary, I not only see reason, from a species of blindness which has happened to myself more than once, to conclude, that a different distribution of nerves takes place in us, but I think my opinion supported by this evident difference of structure in fishes.

It is now more than twenty years since I was first affected with the peculiar state of vision, to which I allude, in consequence of violent exercise I had taken for two or three hours before. I suddenly found that I could see but half the face of a man whom I met; and it was the same with respect to every object I looked at. In attempting to read the name JOHNSON, over a door, I saw only SON; the commencement of the name being wholly obliterated to my view. In this instance the loss of sight was toward my left, and was the same whether I looked with the right eye or the left. This

blindness was not so complete as to amount to absolute blackness, but was a shaded darkness without definite outline. The complaint was of short duration, and in about a quarter of an hour might be said to be wholly gone, having receded with a gradual motion from the center of vision obliquely upwards toward the left.

Since this defect arose from over fatigue, a cause common to many other nervous affections, I saw no reason to apprehend any return of it, and it passed away without need of remedy, without any farther explanation, and without my drawing any useful inference from it.

It is now about fifteen months since a similar affection occurred again to myself, without my being able to assign any cause whatever, or to connect it with any previous or subsequent indisposition. The blindness was first observed, as before, in looking at the face of a person I met, whose *left* eye was to my sight obliterated. My blindness was in this instance the reverse of the former, being to *my right* (instead of the left) of the spot to which my eyes were directed; so that I have no reason to suppose it in any manner connected with the former affection.

The new punctum cæcum was situated alike in both eyes, and at an angle of about three degrees from the center; for, when any object was viewed at the distance of about five yards, the point not seen was about ten inches distant from the point actually looked at.

On this occasion the affection, after having lasted with little alteration for about twenty minutes, was removed suddenly and entirely by the excitement of agreeable news respecting the safe arrival of a friend from a very hazardous enterprise.

In reflecting upon this subject, a certain arrangement of the optic nerves has suggested itself to me, which appears to afford a very probable interpretation of a set of facts, which are not consistent with the generally received hypothesis of the decussation of the optic nerves.

Since the corresponding points of the two eyes sympathise in disease, their sympathy is evidently from structure, not from mere habit of feeling together, as might be inferred, if reference were had to the reception of ordinary impressions alone. Any two corresponding points must be supplied with a pair of filaments from the same nerve, and the seat of a disease in which similar parts of both eyes are affected, must be considered as situated at a distance from the eyes at some place in the course of the nerves where these filaments are still united, and probably in one or the other thalamus nervorum opticorum.

It is plain that the cord, which comes finally to either eye under the name of optic nerve, must be regarded as consisting of two portions, one half from the right thalamus, and the other from the left thalamus nervorum opticorum.

According to this supposition, decussation will take place only between the adjacent halves of the two nerves. That portion of nerve which proceeds from the right thalamus to the right side of the right eye, passes to its destination without interference; and in a similar manner the left thalamus will supply the left side of the left eye with one part of its fibres, while the remaining halves of both nerves in passing over to the eyes of the opposite sides must intersect each other, either with or without intermixture of their fibres.

Now, if we consider rightly the facts discovered by com-

parative anatomy in fishes, we shall find that the crossing of the entire nerves in them to the opposite eyes, is in perfect conformity to this view of the arrangement of the human optic nerves. The relative position of the eyes to each other in the sturgeon, is so exactly back to back, on opposite sides of the head, that they can hardly see the same object; they can have no points which generally receive the same impressions as in us; there are no corresponding points of vision requiring to be supplied with fibres from the same nerve. The eye which sees to the left has its retina solely upon its right side; and this is supplied with an optic nerve arising wholly from the right thalamus; while the left thalamus sends its fibres entirely to the left side of the right eye for the perception of objects situated on the right. In this animal, an injury to the left thalamus might be expected to occasion entire blindness of the right eye alone, and want of perception of objects placed on that side. In ourselves, a similar injury to the left thalamus would occasion blindness (as before) to all objects situated to our right, owing to insensibility of the left half of the retina of both eyes.

A disorder that has occurred within my own knowledge in the case of a friend, seems fully to confirm this reasoning, as far as a single instance can be depended upon. After he had suffered severe pain in his head for some days, about the left temple, and toward the back of the left eye, his vision became considerably impaired, attended with other symptoms indicating a slight compression on the brain.

It was not till after the lapse of three or four weeks that I saw him, and found that, in addition to other affections which need not here be enumerated, he laboured under a defect of

sight similar to those which had happened to myself, but more extensive, and it has unfortunately been far more permanent. In this case the blindness was at that time, and still is, entire, with reference to all objects situated to the right of his center of view. Fortunately, the field of his vision is sufficient for writing perfectly. He sees what he writes, and the pen with which he writes, but not the hand that moves the pen. This affection is, as far as can be observed, the same in both eyes, and consists in an insensibility of the retina on the left side of each eye. It seems most probable, that some effusion took place at the time of the original pain on that side of the head, and has left a permanent compression on the left thalamus. This partial blindness has now lasted so long without sensible amendment, as to make it very doubtful when my friend may recover the complete perception of objects on that side of him.

In reviewing the several phenomena that I have described, we find partial blindness occurring at the same time in both eyes. This sympathy from disease is readily explained, on the supposition that the parts which sympathise receive their nerves from the same source, while the opposite halves of the eyes, which are not at the same time similarly affected, are supplied from an opposite source; and the inference is immediate, that in common vision also the sympathy of corresponding points, which receive similar impressions from the same object, is dependent on the arrangement of nerves thus detected by disease.

We find moreover in the sturgeon, (and it is the same in some other fishes) whose eyes can scarcely see the same object at once, and have no corresponding points which ordi-

narily sympathise, that the two eyes do not receive any nervous fibres from the same source; but one eye receives its nerve wholly from one side, and the other from the other side of the brain.

From the structure of these fish we learn distinctly, that the perception of objects toward one side is dependent on nerves derived from the opposite side of the brain; and in the last case of diseased vision above related, we find apparent injury to one side of the brain, followed by blindness toward the opposite side of the point to which both eyes are directed.

A series of evidence in such apparent harmony throughout, seems clearly to establish that distribution of nerves I have endeavoured to describe, which may be called the semi-decussation of the optic nerves.

On single vision with two eyes.

So long as our consideration of the functions of a pair of eyes is confined to the performance of healthy eyes in common vision, when we remark that only one impression is made upon the mind, though two images are formed at the same moment on corresponding parts of our two eyes, we may rest satisfied in ascribing the apparent unity of the impression to habitual sympathy of the parts, without endeavouring to trace farther the origin of that sympathy, or the reason why, in infancy, the eyes ever assume one certain direction of correspondence in preference to squinting.

But, when we regard sympathy as arising from structure, and dependent on connection of nervous fibres, we therein see a distinct origin of that habit, and have presented to us a

manifest cause why infants first begin to give the corresponding direction to their eyes, and we clearly gain a step in the solution, if not a full explanation, of the long agitated question of single vision with two eyes.

It may perhaps to some persons appear surprising, that so many as three instances of a disorder which they presume to be rare, should have been witnessed by one individual; but I apprehend, on the contrary, this half-blindness to be far more common than is generally supposed; and I might with as much reason express surprise at its having so far escaped notice,* were I not aware how many facts commonly remain disregarded, merely for want of explanation. It is evident that I once, and for a long time, overlooked the inference that is to be drawn from this affection; and if the disorder had not happened to me a second time, I might never have reconsidered its cause.

Even since the preceding pages were written, I have met with two more cases of this disease. One of my friends has been habitually subject to it for sixteen or seventeen years, whenever his stomach is in any considerable degree deranged. In him the blindness has been invariably to his right of the

* RICHTER, in the third volume of his Elements of Surgery, has a chapter on half-blindness, and part of it relates to what he terms amaurosis dimidiata. From one instance there given, he seems to have seen some cases similar to those I have described; but he has not noticed the corresponding affection of the two eyes, or considered the sympathy between them.

Anfangs-gründe Der Wundartzeneykunst.
Vol. 3, Chap. 16, p. 478.

center of vision, and, from want of due consideration, had been considered as temporary insensibility of the right eye; but he is now satisfied that this is not really the case, but that both eyes have been similarly affected with half-blindness. This symptom of his indigestion usually lasts about a quarter of an hour or twenty minutes, and then subsides, without leaving any permanent imperfection of sight.

I have not seen the subject of the 5th case, but I am informed that he has had many returns of this affection, generally attended with head-ach, and always lasting about twenty minutes, with very little variation.

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TRANSACTIONS.

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Read March 4, 1824.

Dear Sir,

As the late expeditions sent out by Government to make discoveries in the Polar regions, were originally planned and recommended by the President and Council of the Royal Society, I am desirous to communicate to the Society, through you, some discoveries that have been made in the line of Comparative Anatomy, from an examination of the specimens that were brought home. This I wish to do before the different expeditions which are to sail in the ensuing spring shall leave our coasts, that the officers employed, knowing that, independent of the great objects of their voyage, many branches of science have been already much enlightened by their means, they will be induced, when-

ever the opportunity occurs, to collect materials by which science may be still farther advanced.

To those who include Comparative Anatomy among their pursuits, it will be gratifying to know that the pickle, or brine, in which the salt provisions are preserved, is well fitted for preserving the internal parts of animals, keeping them in a state better adapted for examination, dissection, and injection, than they are found to be after having long remained in spirit.

The first discovery I shall mention, is a peculiarity in the structure of the hind flipper or foot of the walrus, that has not been adverted to; nor could it have been done now, by any one not well acquainted with the mechanism of the foot of the fly, enabling it to support its weight, and carry on progressive motion against gravity.

Such is the general resemblance between this flipper and the foot of the fly, that having upon a former occasion seen it in a very mutilated state, macerating in water, I discovered this analogy, and requested my friend Captain SABINE, in the Artillery, at the time he sailed with Captain CLAVERING to make experiments on the Figure of the Earth, to bring me the feet and other parts of the walrus. With the assistance and exertions of Mr. ROWLAND, Assistant Surgeon to the ship, he has complied with my request, and enabled me to bring forward the following observations on this subject.

It is a curious circumstance that two animals, so different in size, should have feet so similar in their use. In the fly, the parts require being magnified one hundred times to render this structure distinctly visible; and in the walrus, the parts are so large as to require being reduced four diameters to bring them within the size of a quarto page.

As a knowledge of the structure of the fly's foot, led to the detection of the use of the hind flipper of the walrus; so, on the other hand, an examination of the toes of the walrus has enabled me to make out the use of a part of the foot of the fly which I did not sufficiently understand — I mean the two points; Mr. ADAMS called them pickers, from supposing that they entered certain small holes in the surface, on which progressive motion was carried on. This opinion I did not deem worthy of consideration, but was unable to make out their real use; on comparing them, however, with the outer toes of the walrus, they are evidently intended to surround the exhausted cavity, so that a vacuum may be more suddenly and perfectly formed.

The flipper, whose external appearance is seen, Plate IV. was in a very corrugated state; but in Plate V. in which its muscles and bones are shown, they closely resemble those of the human hand.

The second discovery I have to notice, is the mode in which the bile in the walrus is collected in a reservoir, and thence impelled by a considerable force into the duodenum. The internal surface of the stomach of the walrus consists of rugæ; these in some respects resemble those of the cod fish; the orifice from the œsophagus is very large, so as evidently to admit large masses, and also of regurgitation; in this respect it is so like the seal, as to be distinguished from it only by the difference of size: in both these animals the orifice at the pylorus is extremely small and valvular, preventing the contents of the duodenum from again returning into the stomach. In the seal the gall bladder is small, detached from the liver, and opens, by a very small orifice, two inches and a half from the pylorus. In the walrus, a large cylindrical hard body

lies behind the duodenum, loosely connected to it by cellular membrane; at its lower end it projects like an os tincæ into the gut. This proves not to be the common opening of a ductus communis choledochus, but a canal leading directly from a large oval cavity with thick strong coats, by no means unlike those of the urinary bladder in a thickened state. These parts are shown in Plate VII. half the natural size. This cavity is supplied with bile laterally by a single duct from the liver.

This mode of supplying the duodenum with bile, differs from what is met with in all the animals I have had an opportunity of examining; and what is highly satisfactory, some of the substances on which this animal feeds, are, I believe, almost wholly peculiar to itself. I am informed by my friend Mr. FISHER, who was Astronomer in the two last voyages, that he was present when the contents of a walruses stomach was examined: they consisted entirely of the long branches of sea weed, Fucus digitatus, which is very abundant in the Arctic seas, especially in those parts of them where the walrus is met with in the greatest numbers. One of the seamen said it made him sick to look at the half digested sea weed contained in one of the stomachs. This sea weed, when the sea is open, is thrown up in great quantities on the beach, and when the sea is frozen, is found in masses under the ice.

The mucus secreted by the coats of the gall bladder, which in general is so small as to be of no consideration, in this animal is so abundant, as to induce me to consider it a necessary ingredient to the bile with which it is mixed.

The third new fact, with an account of which I shall con-

clude this letter, is the peculiar structure of the funis and placenta of the seal: for this specimen I am indebted to Lieutenant GRIFFITH, who, during the last voyage, met with it in a seal that was caught. He took out the fœtus and part of the uterus, and brought them home preserved in brine. The preservation of the parts was so complete, that the vessels admitted of being minutely injected.

The placenta in this animal has the following peculiarities: the trunks forming the funis are not twisted together; their whole length is nine inches; three inches from the placenta they begin to give off branches, which freely anastomose with one another; these branches are connected to the placenta itself by three membranous folds, like so many mesenteries; between these folds the blood-vessels are conveyed to the substance of the placenta, on the surface of which they ramify to a great degree of minuteness. This structure will give a greater facility than common to the circulation through the placenta, which makes it an object of enquiry, whether the same peculiarities exist in other marine animals. The drawings of the biliary ducts, and of the placenta, were made by Mr. ROSE, a student in surgery under me at St. George's Hospital.

I am, dear Sir, yours truly,

EVERARD HOME.

Sackville Street,
Feb. 11, 1824.

EXPLANATION OF THE PLATES.

Plate IV. contains four figures, representing different views of the left hind flipper of the walrus, diminished to one-fourth the natural size.

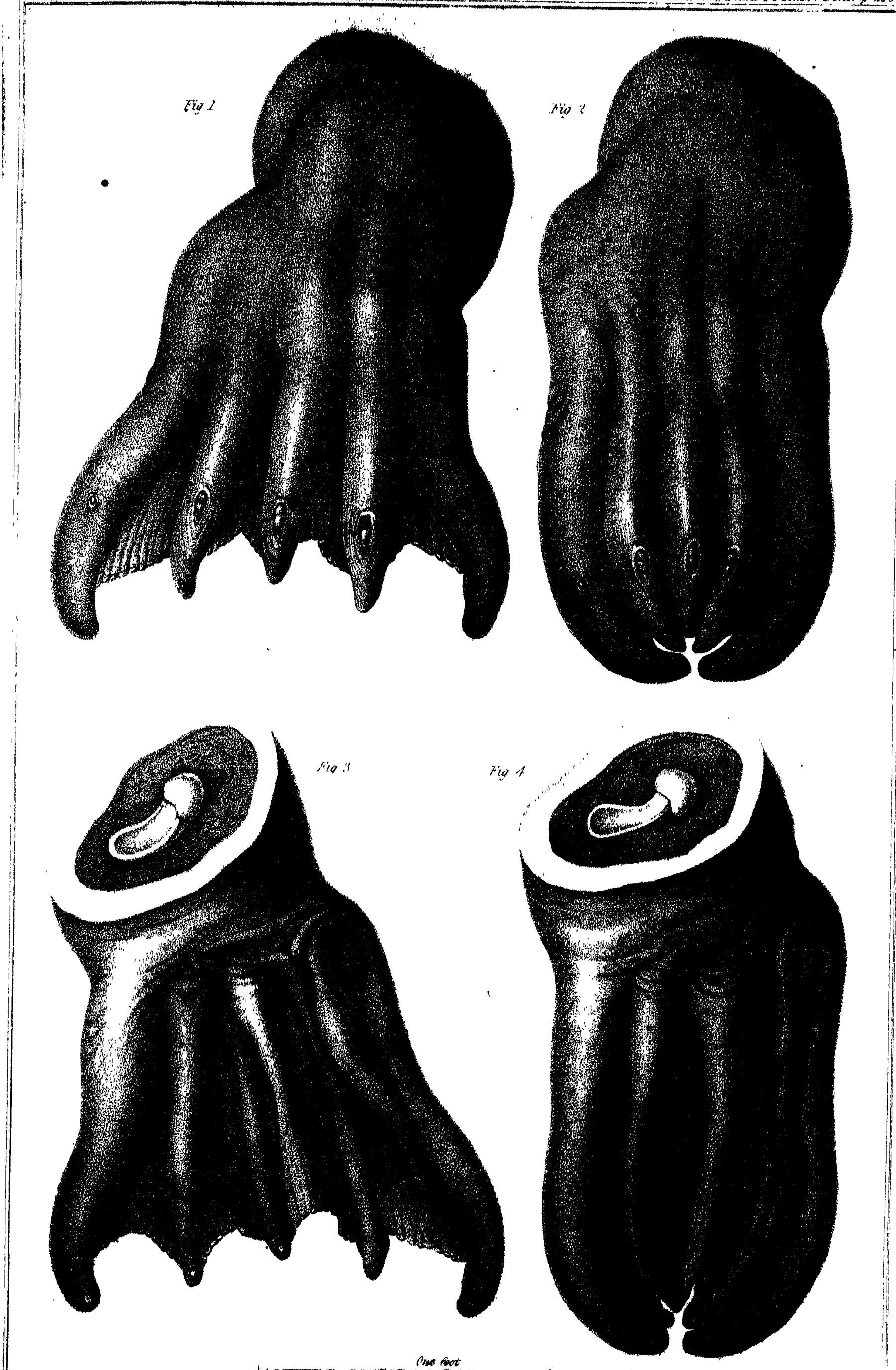
As the skin of the animal is very thick and unyielding, and had been for so long a time in strong brine, the parts were much shrunk and corrugated; but even in this state they showed that the palm of the flipper formed a concavity, which had the appearance of a cup, when the great and little toe were made to encircle the others.

In this state of the parts this concavity was thrown into longitudinal rugæ, so that the real size could not be ascertained, the span from the point of the great toe to the end of the little toe not exceeding twelve inches.

Figs. 1 and 2 represent the flipper, with the palm upon the ground, in the expanded and contracted state.

Figs. 3 and 4 represent the palm in these two states, with the rugæ formed by the skin.

Plate V. shows the internal structure of this flipper, after the thick skin thrown into rugæ upon the palm was dissected off. The flipper now lost all appearance of a foot, and took on that of the hand of a giant, so far as respected the bones and muscles, differing indeed in having a web covering all the other parts, and extending beyond the point of the thumb and fingers. The span now, instead of being twelve inches, became twenty-eight; and although this figure is upon the same scale as those in Plate IV., the span is seven inches.



Franz Bauer del.t

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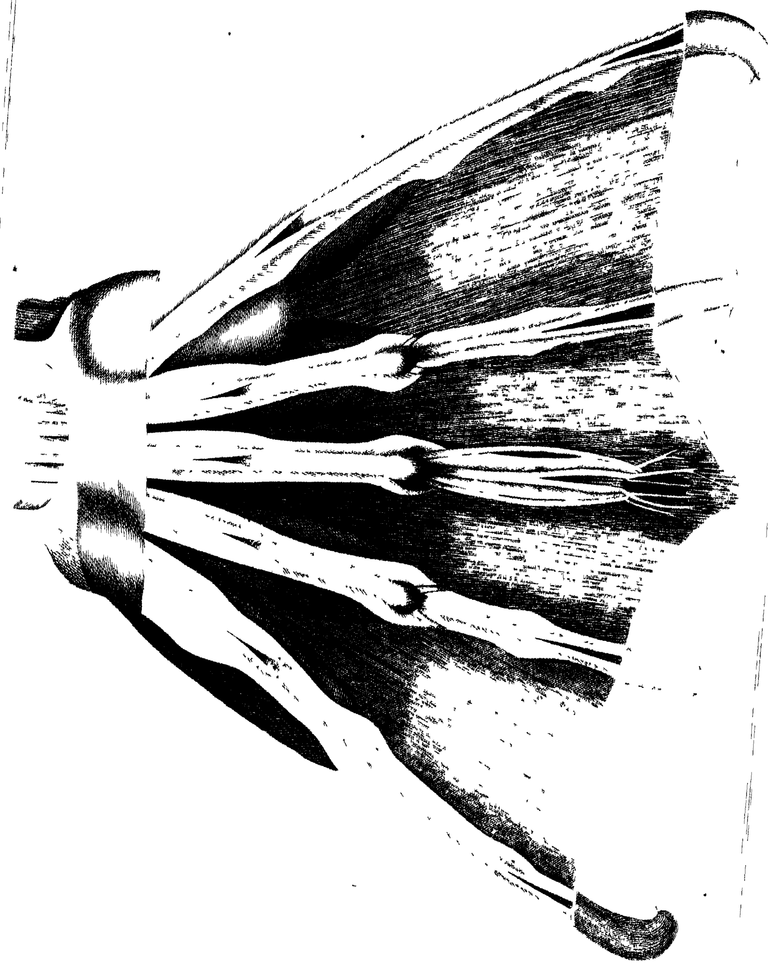

L Rose delt

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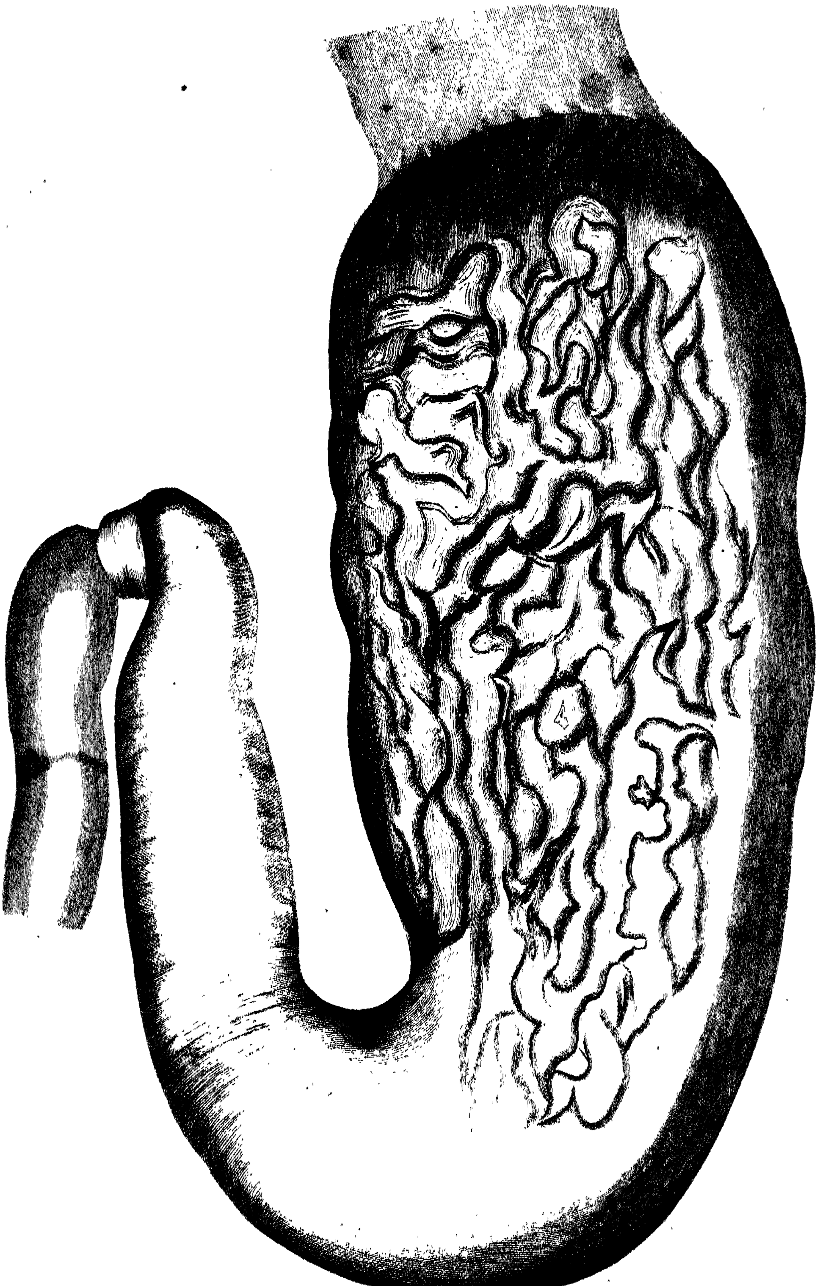
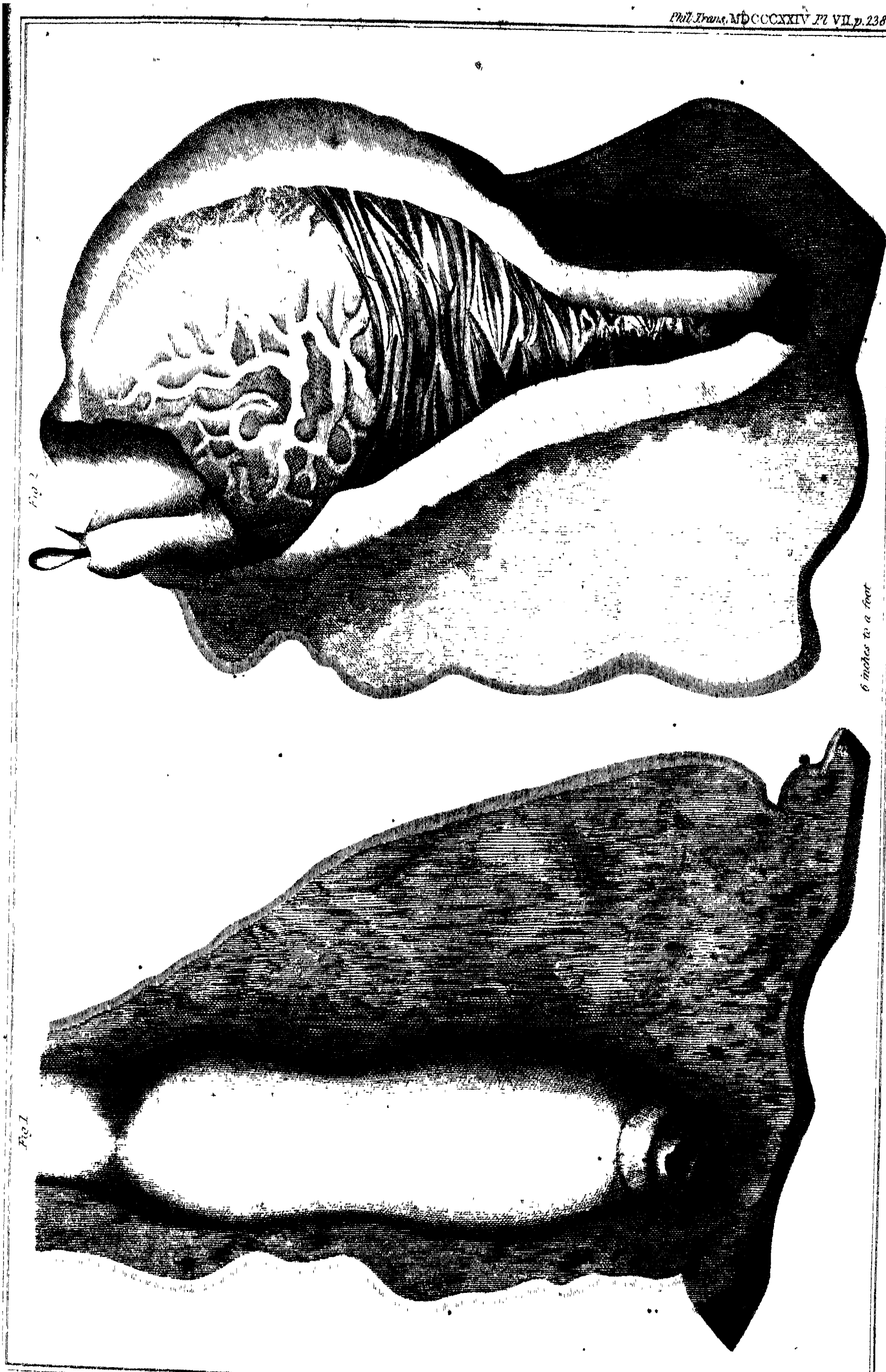


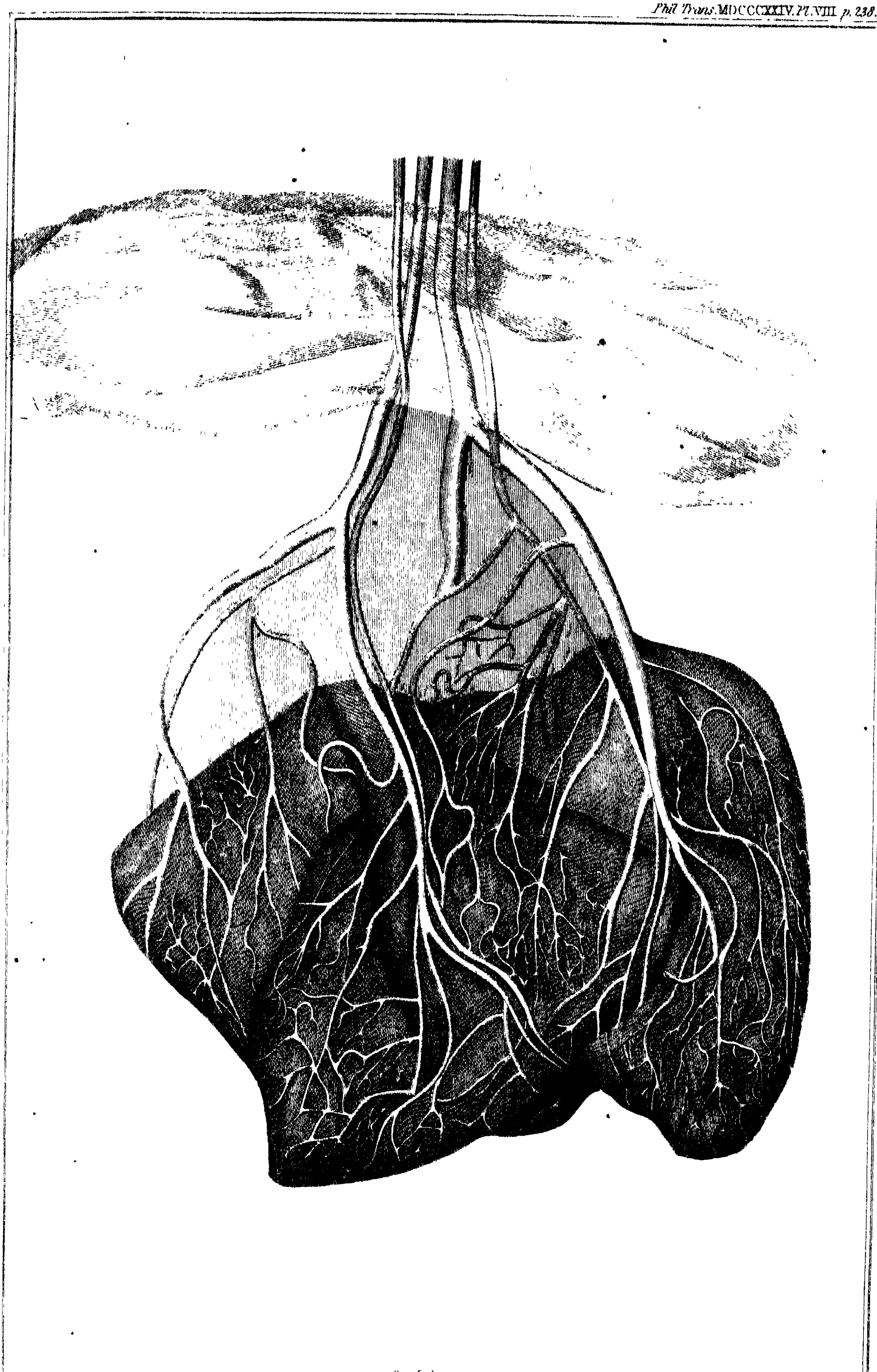
One Inch to a foot

J. Rose del.t J. Basire sculp.t



J. Rose del.t

J. Basire sculp.t



One Inch to two

L. Rose del.t

J. Basire sculp.t

The resemblance of the bones of the hind flipper of a walrus to those of the human hand, which I believe is like nothing else in nature, is curiously exact; the bones of the wrist are the same in number and shape; so are those of the metacarpus; so also the phalanges of the thumb and fingers. The tendons of the perforantes muscles pass through those of the perforati in the palm upon the metacarpal bones, while in the human hand this takes place upon the first phalanges of the fingers; and there are no lumbricales muscles whatever. On the back of this gigantic hand I was astonished to find the tendon of the indicator muscle.

The muscles and tendons that are peculiar to this flipper, not met with in the human hand, are those of the web which extends beyond the fingers and thumb: this web is a strong ligamentous elastic substance intermixed with muscular fibres; it has a set of muscles, which have their origin from the sides of the last phalanges of the fingers insensibly lost in it, and tendons go off from each side of the perforator muscles, which spread out and are lost in it.

That this gigantic hand is employed as a cupping glass to prevent the animal from falling back in its movements, whether on the ice, or in climbing the rocky cliffs, there can be no doubt; for it is only necessary to take the human hand, and envelope it in an elastic web extending some way beyond the points of the fingers, to prove that it could perform such an office; but when we find the lumbricales muscles wanting, the only use of which is to clench the fist, it adds to the proof; and when the indicator is met with, a mode of opening a valve to let the air in is pointed out.

It may be doubted, whether the extent of the flippers is

equal to the support of the enormous bulk of this animal; but this doubt will be removed when I mention, that Mr. FISHER informs me that a walrus, killed at Spitzbergen, weighed twenty hundred weight, and that an exhausted surface of twenty eight inches by twenty will support a pressure of 15lbs. on every square inch, more than double the animal's weight.

That the principle on which the foot of the fly, the gecko, and the walrus, is formed, is the same, I trust has been established. In the fly there are two cups, in the walrus only one.

In Plate VI. the stomach of the walrus, upon the scale of an inch to a foot, is represented inverted, to show its internal surface.

The œsophagus is lined with cuticle, which terminates in a transverse line at the orifice of the stomach; at the pylorus is a valvular fold, and the aperture is contracted and very small.

Plate VII. fig. 1, shows the manner in which the gall duct terminates in the duodenum; the parts are upon a scale of six inches to a foot.

Through the coats of the duodenum the size of the duct is very distinctly seen.

Fig. 2, shows the gall bladder upon the same scale, laid open, as well as the large duct leading to the gut, till it penetrates through its coats.

These parts are distinct from the liver, lying directly behind the duodenum, and connected to it by cellular membrane; the duct by which the bile is brought to this reservoir is also shown.

Plate VIII. represents the placenta of the seal, in which there are several peculiarities; upon a scale of half an inch to an inch: the fœtal surface of the placenta only is exposed.

The funis is nine inches long, but the blood vessels are not twisted on one another as is common in quadrupeds; and after passing three inches from the navel of the fœtus, the vein divides into two branches. At six inches the arteries and veins sub-divide so as to form three distinct chords, and these, instead of going directly into the substance of the placenta, have doublings of membrane surrounding them in form of mesenteries, in which their branches are inclosed; and these terminate in the placenta, rendering the surface very vascular.

This surface of the placenta has a lobulated appearance: but the fissures do not extend to the maternal surface, which has throughout a granulated appearance.

XII. *Additional Experiments and Observations on the Application of Electrical Combinations to the Preservation of the Copper Sheathing of Ships, and to other purposes.* By Sir HUMPHRY DAVY, *Bart. Pres. R. S.*

Read June 17, 1824.

I HAVE already had the honour of communicating to the Royal Society the results of my first researches on the modes of preventing the chemical action of fluid menstrua, such as saline solutions, or sea water containing air, on copper, by the contact of more oxidable metals.

For some months I have been engaged in a series of new experiments on this subject, so important to the navigation and commerce of the country: and through the liberal and enlightened views of Lord MELVILLE, and the Lords of the Admiralty, who desired the Commissioners of the Navy Board and of the Dock Yards to give me every assistance in their power, and all the facilities which our magnificent Naval establishments at Chatham and Portsmouth furnish, I have been enabled to conduct my operations upon a very large scale. At this advanced period of the session, it will be impossible for me to give more than a very short notice of experiments which have been tried under a great variety of circumstances, and the details of which would occupy some hours in reading; but I cannot deprive myself of the pleasure of stating the satisfactory and conclusive nature of

the results, many of which have even surpassed my expectations.

Sheets of copper, defended by from $\frac{1}{40}$ to $\frac{1}{1000}$ part of their surface of zinc, malleable and cast iron, have been exposed, for many weeks, in the flow of the tide in Portsmouth Harbour, and their weights ascertained before and after the experiment. When the metallic protector was from $\frac{1}{40}$ to $\frac{1}{150}$, there was no corrosion nor decay of the copper; with smaller quantities, such as from $\frac{1}{200}$ to $\frac{1}{400}$, the copper underwent a loss of weight, which was greater in proportion as the protector was smaller; and as a proof of the universality of the principle, it was found that even $\frac{1}{1000}$ part of cast iron saved a certain proportion of the copper.

The sheeting of boats and ships, protected by the contact of zinc, cast and malleable iron in different proportions, compared with those of similar boats and sides of ships unprotected, exhibited bright surfaces, whilst the unprotected copper underwent rapid corrosion, becoming first red, then green, and losing a part of its substance in scales.

Fortunately, in the course of these experiments, it has been proved that cast iron, the substance which is cheapest and most easily procured, is likewise most fitted for the protection of the copper. It lasts longer than malleable iron, or zinc; and the plumbaginous substance, which is left by the action of sea water upon it, retains the original form of the iron, and does not impede the electrical action of the remaining metal.

I had anticipated the deposition of alkaline substances in certain cases upon the negatively electrical copper. This has actually happened. Some sheets of copper, that have

been exposed nearly four months to the action of sea water, defended by from $\frac{1}{35}$ to $\frac{1}{80}$ of their surface of zinc and iron, have become coated with a white matter, which, on analysis, has proved to be principally carbonated lime, and carbonate and hydrate of magnesia. The same thing has occurred with two harbour boats, one of which was defended by a band of zinc, the other by a band of iron, equal to about $\frac{1}{35}$ of the surface of the copper.

These sheets and boats remained perfectly clean for many weeks, as long as the metallic surface of the copper was exposed; but lately, since it has become coated with carbonate of lime and magnesia, weeds have adhered to these coatings, and insects collected on them; but on the sheets of copper, defended by quantities of cast iron and zinc, bearing a proportion below $\frac{1}{150}$, the electrical power of the copper being less negative, more neutralised, and nearly in equilibrio with that of the menstruum, no such effect of deposition of alkaline matter or adherence of weeds has taken place, and the surface, though it has undergone a slight degree of solution, has remained perfectly clean: a circumstance of great importance, as it points out the *limits* of *protection;* and makes the application of a *very small* quantity of the oxidable metal, more advantageous in fact than that of a larger one.

The wear of cast iron is not so rapid; but that a mass of two or three inches in thickness will last for some years. At least the consumption in experiments which have been going on for nearly four months, does not indicate a higher ratio. This must however depend on the relation of its mass to that of the copper, and upon other circumstances not yet ascertained (such as temperature, the relative saltness of the sea,

and perhaps the rapidity of the motion of the ship;) circumstances in relation to which I am about to make decisive experiments.

Many singular facts have occurred in the course of these researches. I shall mention some of them, that I have confirmed by repeated experiments, and which have connections with general science.

Weak solutions of salt act strongly upon copper; strong ones, as brine, do not affect it; and the reason seems to be, that they contain little or no atmospheric air, the oxygene of which seems necessary to give the electro-positive principle of change to menstrua of this class.

I had anticipated the result of this experiment, and upon the same principle of some others.

Alkaline solutions, for instance, impede or prevent the action of sea water on copper; having in themselves the positive electrical energy, which renders the copper negative. Lime water even, in this way, renders null the power of action of copper on sea water.*

The tendency of electrical and chemical action being always to produce an equilibrium in the electrical powers, the agency of all combinations formed of metals and fluids is to occasion decompositions, in such an order that alkaline, metallic, and inflammable matters are determined to the negative part of the combination, and chlorine, iodine, oxygene and acid matters to the positive part. I have shown in the Bakerian Lecture for 1806, that this holds good in the Voltaic battery. The same law applies to these feebler

* I am at present engaged in applying this principle to experiments on the preservation of animal and vegetable substances.

combinations. If copper in contact with cast iron be placed in a vessel half full of sea water, and having its surface partially above that of the water, it will become coated with carbonate of lime, carbonate of magnesia, and carbonate of soda; and the carbonate of soda will gradually accumulate till the whole surface in the air is covered with its crystals:—and if the iron is in one vessel, and the copper forming an arc with it in another; and a third vessel of sea water in electrical connection by asbestos or cotton is intermediate, the water in this intermediate vessel continually becomes less saline; and undoubtedly, by a continuance of the process, might be rendered fresh.

I shall not take up the time of the Society, by referring to some obvious practical applications of these researches, to the preservation of finely divided astronomical instruments of brass by iron, of instruments of steel by iron, or zinc: my friend Mr. PEPYS has already ingeniously taken advantage of this last circumstance, in inclosing finely cutting instruments in handles or cases lined with zinc, and many other such applications will occur. I cannot conclude, without mentioning particularly my obligations to Sir BYAM MARTIN, the Comptroller, and Sir ROBERT SEPPINGS, the Surveyor of the Navy, for the interest they have taken, and the zeal they have shown in promoting these researches; and without stating how much I owe to the care, attention, and accuracy of Mr. NOLLOTH, Master Ship-wright, and Mr. GOODRICH, Mechanist in the Dock-yard at Portsmouth, in superintending the execution of many of the experiments.

XIII. *On the Apparent Direction of Eyes in a Portrait.* By William Hyde Wollaston, *M. D. F. R. S. and V. P.*

Read May 27, 1824.

It may seem, at first view, that portrait painting is not altogether a fit subject to be brought before the Royal Society, since the delicate touches by which the skill and feeling of an accomplished artist convey an expression of sense, and grace, and sensibility to the finished representation of the human form, cannot admit of such strict analysis as the ordinary subjects of our inquiry.

Nevertheless, since the rules of perspective, which are strictly mathematical, are perfectly within our province, it may be presumed that a question, in which some principles of that science are involved, may be considered a legitimate subject of communication; that effects not anticipated on any received principles must deserve attention; and that the explanation of them will be found to have some pretensions to utility.

When we consider the precision, with which we commonly judge whether the eyes of another person are fixed upon ourselves, and the immediateness of our perception that even a momentary glance is turned upon us, it is very surprising that the grounds of so accurate a judgement are not distinctly known, and that most persons, in attempting to explain the subject, would overlook some of the circumstances by which, it will appear, they are generally guided.

Though it may not be possible to demonstrate, by any decisive experiment on the eyes of living persons, what those circumstances are, still we may find convincing arguments to prove their influence, if it can be shown in the case of portraits, that the same ready decision we pronounce on the direction of the eyes is founded, in great measure, on the view of parts which, as far as I can learn, have not been considered as assisting our judgement.

Previous to a full examination of this question, one might imagine that the circular form of the iris would be a sufficient criterion of the direction in which an eye is looking, since, when the living eye is pointed to us, this part is always circular, but cannot appear strictly so, when turned in such a manner that we view it with any degree of obliquity. But, upon farther consideration, it is evident that we cannot judge of exact circularity with sufficient precision for this purpose, even when the whole circle is fully seen, and in many cases we see too small a portion of the circumference of the iris to distinguish whether it is circular or elliptic.

Moreover, in a portrait, although the iris be drawn most truly circular, and consequently will appear so when we have a direct view of it, still, in all oblique positions, it must be seen as an ellipse. And yet the eyes, as is well known, apparently continue to look at the spectator, even when he moves to view them very obliquely, and sees them of a form most decidedly elliptic.

The reason why the eyes of a portrait seem to follow us will be hereafter considered, but cannot be rightly explained until the circumstances, on which apparent direction in the front view depends, are fully understood.

If we examine with attention the eyes of a person opposite to us, looking horizontally within about twenty degrees on either side of us, we find that the most perceptible variation in the appearance of his eyes, in consequence of their lateral motion, is an increase and decrease of the white parts at the angles of each eye, dependent on their being turned to or from the nose.

In the central position of an eye, the two portions of white are nearly equal. By this equality, we are able to decide that a person is looking neither to his right nor to his left, but straight forward in the direction of his nose, as index of the general position of his face.

If, on the contrary, he turn his eyes to one side, we are immediately made sensible of the change by a diminution of the white of the eye on that side to which they turn, and by this test alone we are able to estimate in what degree they deviate in *direction from the face to which they belong.*

But *their direction with reference to ourselves* is perfectly distinct from the former; and in judging of this it seems probable that, even in viewing real eyes, we are not guided by the eyes alone, but are unconsciously aided by the concurrent position of the entire face; for in a portrait, the effect of this further condition admits of being proved by a distinct and decisive experiment.

If a pair of eyes be drawn with correctness, looking at the spectator, at such moderate deviation from the general position of the face as is usual in the best portraits, unless some touch be added to suggest the turn of face, the direction of the eyes seems vague, and so undetermined, that their direction will not appear the same to all persons; and to the same

person they may be made appear directed either to him or from him by the addition of other features strongly marking that essential circumstance, the *position of the face.*

In the drawings which I am enabled to exhibit to the Society, I am indebted for assistance to the well known skill and obliging kindness of Sir THOMAS LAWRENCE, President of the Royal Academy, by whom the pair of eyes represented in the first plate were originally drawn from the life, intently looking at him. To these a turn of face has since been added according to the original design, so that the eyes, with this accompaniment, Fig. 1, appear decidedly looking at the spectator.

In Fig. 2. a set of features oppositely turned are so applied to the same eyes, that they look considerably to the right of the person viewing them.*

In the former of these, the position of the face being at a certain angle to our left, the eyes, which are turned at an equal angle from that position, seem pointed to ourselves. In the latter, the deviation of the face from us being toward the same side as the turn of the eyes, gives additional obliquity to their apparent direction, and carries them far to the right of us, proving the influence of the stronger features, even in opposition to that of the minuter parts of the eyes themselves, which are not in correct drawing for this position.

With regard to the apparent position *of the face*, it is clear that, in forming our judgement, we must be influenced princi-

* The effect of this change is so sudden, and so contrary to expectation, that, at first sight, many persons seem scarcely to credit the evidence of their senses, in supposed opposition to their former experience, and are inclined to imagine some present *deception* in the very phenomena best adapted to *undeceive* them as to the cause of the impression they receive.

pally by the nose and other parts of it that are most prominent, because these, in nature, are subject to the greatest changes of perspective form by any alteration of position; and we scarcely notice those smaller variations of figure, to which even parts least prominent are liable when seen very obliquely.

It must be obvious to the most superficial observer, that the same perspective form which correctly represents a certain pair of eyes in one position of the face, cannot be an exact representation of the *same* eyes in another; but in cases of such slight obliquity as is usually given to the eyes in a portrait that is intended to look at the spectator, the variation of the form of the lids from obliquity is less than the difference observable in the eyes of different persons. Hence it is that a pair of eyes drawn looking at us, will best admit of being warped from their intended direction by application of a new position of the other features of the face.

The converse of this experiment may also be made with success within the same limited extent. Eyes drawn originally looking a little to one side of us, may be made to look at us by applying other features in a suitable position. But although a change of twenty or perhaps thirty degrees may be effected, it is not to be supposed that a turn of ninety degrees can be produced. It would be absurd to imagine that an eye drawn in profile could be made to look full upon us, or that an eye looking nearly at us could be made to appear in profile.

If an attempt be made to carry the experiment beyond reasonable limits, so that the perspective form of the eyes is

glaringly ill-suited to that of the rest of the face, the effect is impaired by such obvious discordance, but is not altogether lost; for though some persons much accustomed to drawing the human eye, who are in the habit of attending minutely to the shape of the lids, may not feel the full effect perceived by others, still the change of direction that *is* admitted by the generality of those who have nothing to warp their judgement, shows how little influence the eyelids really have in giving apparent direction, in comparison with the more prominent features.

In order to show how small an addition is sufficient to produce the effect, in Plate X. are four copies of another representation of the same pair of eyes made exactly alike by the admirably ingenious process of Mr. Perkins. A strong plate of steel on which they were first engraved, having been subsequently hardened, gave an elevated impression of them to a soft steel roller, passed with great force repeatedly over the surface of the plate. The roller having next been hardened in its turn, became the tool for transferring four impressions to the same plate of copper, with the most unquestionable identity of representation in the four copies to each other. Nevertheless in two of these their apparent direction will be seen to differ by the mere position of the noses, and in the others a corresponding difference is effected solely by means of the upper half of the face.

For the sake of greater perspicuity, we have hitherto considered merely the cases of *lateral* turn of the eyes and face, at small angles of deviation to the right or left, by the balance of which, if in opposite directions, the eyes appear to look at us; or, if the inclination of both be toward the same side of

us, then the eyes seem turned away from us by the sum of those angles.

The same principles apply also to instances of moderate inclination of the face upwards or downwards. For when the face is pointed downwards, the eyes that look at us must be turned upwards from the position of the face to which they belong. And, if to eyes so drawn an upward cast of features be substituted for the former, the eyes seem immediately to look above us.

When the turn of a pair of eyes partakes of both inclinations, so as to be in a direction laterally upwards, the alteration produced by changing the position of the face, affords the most striking exemplification of the force of this principle, as may be seen in Plate XI, and its companion.

But the effect thus producible is by no means limited to the mere extent of deviation, as a total difference of character may be given to the same eyes by due representation of the other features. A lost look of devout abstraction in an uplifted countenance may be exchanged for an appearance of inquisitive archness, in the leer of a younger face, turned downwards and obliquely toward the opposite side. The under eyelid, which in the former position conceals a portion of the ball of the eye, from an effect apparently of mere perspective, will in the latter seem raised with effort, and thus give the appearance of a smile to the same eyes, if supported by corresponding expression of the rest of the countenance. But it is needless to pursue the various modifications of which this experiment is obviously susceptible. The instances already given are sufficient to show that the apparent direction of the eyes to or from the spectator depends upon the

balance of two circumstances combined in the same representation, namely,

1st. The general position of the face presented to the spectator; and,

2dly. The turn of the eyes from that position.

With this previous knowledge of the influence which the general perspective of the face in a portrait, has upon the apparent direction of the eyes, we shall be prepared to examine why, if they look at the spectator when he stands in front of the picture, they follow, and appear to look at him, in every other direction.

If we consider the effect produced by our change of position with reference to any other perspective drawing, we find a similar permanence of apparent position of the objects represented with respect to ourselves, and corresponding change of direction with reference to the plane of representation, or to the room in which it hangs; and we shall be able, in this case, distinctly to trace its origin in the simplest principles of perspective drawing.

When two objects are seen on the ground at different distances from us in the same direction, one will appear and must be represented exactly above the other. The line joining them is an upright line on the plane of the picture, and represents a vertical plane passing through the eye and these objects. When objects that are at different elevations are said to be in a line with us, the strict meaning is, that they are so placed that a vertical plane from the eye would pass

through them. Now, since the upright line (drawn or supposed to be drawn on the plane of the picture and representing a vertical plane) will be seen upright, however far we move to one side, and will continue to represent a vertical plane, it follows that the same set of objects, even in the most oblique direction in which the representation can be viewed, are still in the same vertical plane, and consequently will seem still to be in a line with us, exactly as in the front view: seeming as we move, to turn round with us, from their first direction, toward any oblique position that we may choose to assume.

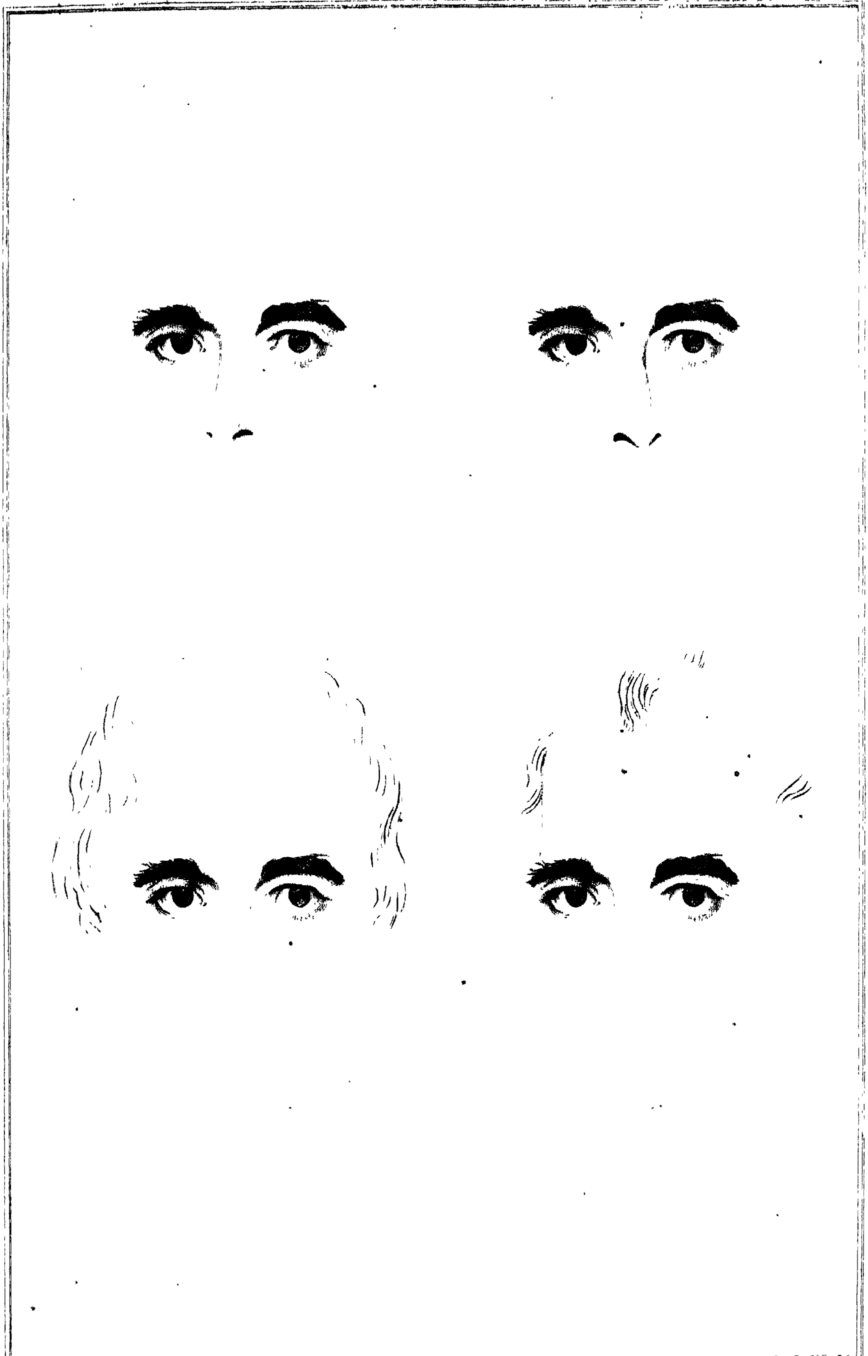
In portraits, the phenomena of direction with reference to the spectator, and corresponding change of apparent position in space when he moves to either side, depend precisely on the same principles. A nose drawn directly in front with its central line upright, continues directed to the spectator, though viewed obliquely. Or, if the right side of the nose is represented, it must appear directed to the right of the spectator in all situations; and eyes that turn in a due degree from that direction toward the spectator, so as to look at him when viewed in front, will continue to do so when viewed obliquely.

As an illustration of the permanent directions of the nose and eyes in a portrait, if a compass be represented, Plate XII. in front of the picture, in a square box, so placed that the sides appear in the same direction as the nose, the needle being set parallel to that of the eyes, will represent, in all positions from which it can be viewed, a line pointing in their apparent direction, and by its permanently vertical position will justly exhibit the same appearance which eyes do, of

following the spectator to either side. In the same manner, if the eyes be turned toward one side, a corresponding needle would duly appear to retain its position toward the same side of the spectator, just as the box does in the former instance.

In any extended drawing the lines of direction admit of being clearly marked in the relative position of objects at different distances; but in portraits the circumstances are less distinct, for want of some visible mark indicating the direction of the eyes. But, if any object be represented in front of the picture, so that the center of one of the eyes may appear to be exactly over it, we have then a marked line of direction, which, by its permanently vertical position, renders the relation of the appearances in a portrait, to the corresponding phenomena in extended views, complete.

Fig 1



John Shaw

XIV. *Farther particulars of a case of Pneumato-thorax. By* JOHN DAVY, *M. D. F. R. S.*

Read March 4, 1824.

IN the last communication which I had the honour of making to the Royal Society on the subject of pneumato-thorax, I expressed the hope that the patient, who had been operated on, and greatly relieved by tapping the chest, and permitting the accumulated air to escape, would eventually recover. This anticipation, I regret I cannot confirm. The case has terminated fatally; and I now beg leave to describe briefly its progress.

On the 17th of June, about a month after the operation, when the patient appeared to be doing so well, there were symptoms indicating the supervention of hydro-thorax;* and, in another week, these symptoms had so much increased, that they could not be mistaken. Avoiding minute details, it will be sufficient to observe that the patient, when he attempted to lie on the right side, was instantly seized with a fit of coughing; and that when his body was shaken, the sound of fluid fluctuating in air in the left side of the chest was distinctly audible even at the distance of several yards.

As the patients' health was pretty good, it was deemed advisable, in consultation, to repeat the operation of paracentesis, and draw off the fluid, which was rapidly increasing, before the case should become desperate.

* Vide Philosophical Transactions for 1822, p. 512.

Having, in cases of empyema, found many inconveniences attending the puncturing the pleura between an intercostal space, I was induced, in this instance, to follow the method described by HIPPOCRATES, of perforating one of the ribs.* The fifth rib was selected; and having cut down upon it with a scalpel just below the papilla of the breast, I bored through its substance with a carpenter's auger, and punctured the pleura with a small trochar, as nearly as possible of the same size as the auger. On withdrawing the stilette, it was followed by a stream of transparent fluid, fourteen ounces of which were allowed to escape; and then leaving the canula in the perforation, it was closed with a cork, and secured by proper dressings.

Daily, during six weeks, more or less fluid was discharged through an opening in the rib, amounting altogether to rather more than twenty pints. At first, the fluid was transparent, of specific gravity 1021; it coagulated when heated, and contained some alkali in the state of subcarbonate. In a few days pus appeared in it, and gradually increased in quantity till the 15th of July, when the discharge was almost entirely purulent; after which, the proportions of pus and serum varied considerably; one sometimes predominating, and sometimes the other. It may be deserving of notice, that at no time was I able to detect free carbonic acid in the fluid discharged.

Though from the sound of fluctuation in the chest, air was evidently contained in the pleura, yet none escaped with the fluid during the first fortnight; after which a considerable quantity was daily expelled. By means of the perforation in

* Hippocrat. de intern. affect. cap. xxiv.

the rib, and using a flaccid bladder furnished with a trochar, I could at any time collect this air for examination with the greatest ease. The first portion collected (about twenty cubic inches), was, on the 15th of July, examined by means of lime water and phosphorus; it was found to consist of

7.5 carbonic acid gas,
2.5 oxygene,
90.0 azote.

The second portion collected (about thirty-five cubic inches) was on the 20th July; it consisted of

6.0 carbonic acid gas,
5.5 oxygene,
88.5 azote.

The last portion collected was on the 29th of the same month (about forty cubic inches); it consisted of

8.0 carbonic acid gas,
4.0 oxygene,
88.0 azote.

For some days after the operation of paracentesis, the health of the patient deteriorated; his appetite diminished; and he had some fever, but unattended with rigors. At this time, judging from the increasing proportion of pus that appeared in the fluid dicharged, the pleura was probably undergoing inflammation, though the side affected was quite free from pain. Gradually these symptoms subsided; and on the 15th July he felt better than before the operation; he had less difficulty of breathing, very little cough, and he could lie easily on either side. This improvement was progressive till the 23rd July, after which his health again became worse, his appetite impaired, and his spirits low; he had

slight fever and a feeble pulse, varying in frequency between 90 and 120. This unfavourable change was attended with the emission of a large quantity of air from the pleura, and with an alteration in the character of the fluid discharged, which had become more purulent, of a greenish hue, and of an offensive odour. The patient expired suddenly on the 29th July.

The body was examined twelve hours after death, when the following were the most important morbid appearances that were discovered.

The cavity of the abdomen having been laid open, the diaphragm was found slightly protruding into the left hypochondriac region, without displacement of any of the abdominal viscera.

The body having been immersed in a bath, on opening into the left pleura between the first and second ribs, air, to the amount of 170 cubic inches issued out and was collected: it consisted of

1.6 carbonic acid gas,
1.5 oxygene,
82.5 azote.

The left pleura contained besides, about six ounces of pus, so much having subsided from the water that entered the chest to supply the place of the air.

The right pleura was quite free from disease. The right lung appeared to be healthy, but on minute examination, numerous granular translucent tubercles were detected disseminated through its substance, and two small vomicæ were found in the upper part of its superior lobe. The heart was displaced; it was situated on the spine, inclining a little to the right side, and the position of the greater part of the

œsophagus was similar. The pericardium was firmly attached to the middle lobe of the right lung by a firm band of adhesion. On exposing to view the left cavity of the chest, the inside of the pleura exhibited a surface of milk-white granular coagulable lymph, about two lines thick, equally diffused on the costal and the pulmonic side. Excepting the cicatrices externally in the skin, no traces could be detected of the two first operations, nor was there any mark of the last operation, exclusive of the small opening, which had been carefully kept open, just large enough to admit the trochar, which had been daily introduced to draw off the fluid, and allow the air to escape. On maceration of the rib, it may be remarked, a very narrow ring of bone was found exfoliating from the perforated part. The left lung was very much condensed, and so firmly confined by its thickened pleura, that it did not dilate when air was driven into it with some force by a double bellows attached to the trachea. This experiment was made with the lung under water, for the purpose of ascertaining if any, and what kind of communication, existed between the lung and the pleura, with a view to discover the origin of the air accumulated in this cavity. Two communications were thus detected; one in the inferior, the other in the superior lobe. The former was so exceedingly small that it could not be traced. The latter opening was sufficiently large to admit a surgeon's probe, and its course was easily followed; it was found to communicate directly and obliquely with a ruptured opening in the side of a large bronchial tube, situated immediately under the pleura. The adjoining pulmonary substance appeared to be merely condensed from compression. The

substance of the lung generally, was in the same state ; and, besides containing a very few minute tubercles, it exhibited no other marks of disease.

In the fatal case of IREDILL, described in my former paper, pneumato-thorax took place in consequence of ulceration effecting a communication between the cavity of the pleura and a vomica in the lung. In this instance the disease originated without the intervention of a vomica, and probably without ulceration ; it appears to have resulted from a communication between the aspera arteria and cavity of the pleura, established by the rupture of a superficial bronchial tube, and the membrane of the pleura covering it. It is surprising that accidents of this kind do not more frequently occur, considering the very large number of bronchial tubes that lie immediately under the pleural covering of the lung, and how delicate this membrane is, and how easily both it and the bronchia are torn.

In a professional point of view, it would be an interesting, though not an easy task, to trace the different steps of the disease, of which I have given a brief history, from its commencement to its termination, and connect the symptoms with the organic changes that occurred. As more appropriate to this place, I shall confine the few remarks I have farther to make, to the air procured from the pleura. The following table exhibits, at one view, the composition of the air collected from the chest at different times.

When collected.	Composition.		
	Carbonic acid.	Oxygene.	Azote.
May 21	7	—	93
July 15	7.5	2.5	90
—— 20	6	5.5	88.5
—— 29	8	4	88
—— 30	16	1.5	82.5

To what are these variations in composition, which the table exhibits, owing? I cannot conceive that they depend entirely on the admission of variable quantities of atmospheric air by the external opening, because exceedingly little atmospheric air could enter through that channel, both from the great care taken to exclude it, and from the valvular nature of the passage.* I believe we must look chiefly to the source of the air and the absorbent power of the pleura for the explanation in question. In this case, as in IREDILL's, there is reason to suppose that the air accumulated in the chest was common air, more or less vitiated by respiration previously, and more or less altered by the process of absorption after entering the cavity of the pleura. Taking this view of the subject, the composition of the air each time it was examined, is easily accounted for, excepting in the last instance, when the proportion of carbonic acid gas was found

* The perforation being slightly oblique, the pleura costalis lined with coagulable lymph, closed the internal aperture in the rib on expiration, and prevented completely the egress of air, even when the dressings were removed, and the external aperture uncovered. When the trochar was introduced, the stilette was withdrawn during expiration, and the finger was applied to the mouth of the canula during each inspiration.

to be so large, *post mortem*. On what this depended, it is not easy to say; it is matter for conjecture, and seems to require farther investigation. Messrs. ALLEN and PEPYS, after a forced expiration, found air from the lungs to contain as much as 9.5 per cent. carbonic acid gas;* and, in different instances that I have examined the air contained in the lungs a few hours after death, I have found the proportion of carbonic acid gas to vary from 8 to 12 per cent.; thus, in a fatal case of empyema, the air procured from one lung that was sound, consisted of

8.3 carbonic acid gas,
5.0 oxygene,
86.7 azote:

that from the other lung, which was condensed, and as it were hepatized, of

12.5 carbonic acid gas,
2.0 oxygene,
85.5 azote:

whilst in another case, in which one lung was sound, and the other abounded in minute cavities full of pus, air from the sound lung consisted of

12.2 carbonic acid gas,
3.0 oxygene,
84.8 azote.

Had the proportion of carbonic acid gas, in the instance under consideration, been within these limits, the explanation would have been attended with little difficulty;

* Philosophical Transactions, 1808.

but, exceeding these limits, one is almost disposed to refer it to exhalation or secretion from the pleura, a notion in favour of which, some facts, were it necessary, might be adduced.

Edinburgh
December 4, 1823.

XVI. *On the action of finely divided Platinum on Gaseous Mixtures, and its Application to their Analysis.* By William Henry, *M. D. F. R. S.*

Read June 17, 1824.

Several years have elapsed since the President of the Royal Society, in the further prosecution of those Researches on Flame, which had already led him to the most important practical results, discovered some new and curious phenomena in the combustion of mixed gases, by means of fine wires of platinum introduced into them at a temperature below ignition. A wire of this sort being heated much below the point of visible redness, and immersed in a mixture of coal gas and oxygen gas in due proportions, immediately became white hot, and continued to glow until all that was inflammable in the mixture was consumed. The wire, repeatedly taken out of the mixture and suffered to cool below the point of redness, instantly recovered its temperature on being again plunged into the mixed gases. The same phenomena were produced in mixtures of oxygen with olefiant gas, with carbonic oxide, with cyanogen, and with hydrogen; and in the last case there was an evident production of water. When the wire was very fine, and the gases had been mixed in explosive proportions, the heat of the wire became sufficiently intense to cause them to detonate. In mixtures, which were non-explosive from the redundancy of one or other gas, the combination of their bases went on silently, and the

same chemical compounds were formed as by their rapid combustion.*

Facts analogous to these were announced, in the autumn of last year, by Professor DOBEREINER of Jena, with this additional and striking circumstance, that when platinum in a spongy form is introduced into an explosive mixture of oxygen and hydrogen, the metal, even though its temperature had not been previously raised, immediately glows, and causes the union of the two gases to take place, sometimes silently, at others with detonation. It is remarkable, however, that platinum in this form, though so active on mixtures of oxygen and hydrogen, produces no effect, at common temperatures, on mixtures of oxygen with those compound gases, which were found by Sir HUMPHRY DAVY to be so readily acted upon by the heated wire.† Carbonic oxide appears, indeed, from the statement of M. M. DULONG and THENARD,‡ to be capable of uniting with oxygen at the temperature of the atmosphere, by means of the sponge; but though this is in strictness true, yet the combination, in all the experiments I have made, has been extremely slow, and the due diminution of volume has not been completed till several days have elapsed. On mixtures of olefiant gas, of carburetted hydrogen, or of cyanogen, with oxygen, the sponge does not, by any duration of contact, exert the smallest action at common temperatures.

It was this inefficiency of the platinum sponge on the compounds of charcoal and hydrogen in mixture with oxygen,

* Philosophical Transactions, 1817, p. 77.

† DOBEREINER in Ann. de Chim. et de Phys. XXIV—XCVI.

‡ Ditto. XXIII. 442.

while it acts so remarkably on common hydrogen, and also, though slowly, on carbonic oxide, that suggested to me the possibility of solving, by its means, some interesting problems in gaseous analysis. I hoped, more especially, to be able to separate from each other the gases constituting certain mixtures to the compositions of which approximations only had been hitherto made, by comparing the phenomena and results of their combustion, with those which ought to ensue, supposing such mixtures to consist of certain hypothetical proportions of known gases. It might, for instance, be expected, that from a mixture of hydrogen and carburretted hydrogen with oxygen, the platinum sponge would cause the removal of the hydrogen, leaving the carburetted hydrogen unaltered. To ascertain this, and a variety of similar facts, I made artificial mixtures of the combustible gases in known volumes; and submitted them, mixed with oxygen, sometimes to contact with the sponge, and sometimes with the balls made of clay and platinum, described by Professor DOBEREINER.*

* The proportions which I used, but which perhaps are not of much importance, were two parts of fine china clay, and three parts of spongy platinum mixed with water into a paste, which was moulded into small spherules, about the size of peas. The sponge, best adapted to the purpose of acting on mixed gasses, is obtained by using a little pressure to the ammonia-muriate, after putting it into the crucible If too light and porous, the sponge is apt to absorb mercury by being repeatedly passed through it, and to become amalgamated. In order that the balls or sponge might be removed after their full action, they were fastened to pieces of platinum wire.

SECTION I.

ON THE ACTION OF FINELY DIVIDED PLATINUM ON GASEOUS MIXTURES AT COMMON TEMPERATURES.

I. *Mixtures of Hydrogen and Olefiant Gases with Oxygen.*

When to equal volumes of olefiant gas, and an explosive mixture (which is to be understood, whenever it is so named, as consisting of two volumes of hydrogen and one of oxygen gases) one of the platinum balls, recently heated by the blow-pipe, and allowed to cool during eight or ten seconds, is introduced through mercury, a rapid diminution of volume takes place; the whole of the hydrogen and oxygen gases is condensed; but the olefiant gas is either not at all, or very little acted upon. In a few experiments, when the tube was narrow, and the quantity of mixed gases small, the olefiant gas escaped combustion entirely; but, in general, an eighth or tenth of it was converted into water and carbonic acid. It is difficult, however, to state the precise proportion of any gas which, when added to an explosive mixture, renders the latter insensible to the action of the balls or sponge; for much depends on their temperature when introduced into the gaseous mixture, the diameter of the containing vessel, and other circumstances, which, in comparing different gases, should be so regulated as to be equal in every case.

When the proportions of the gases are changed, so that the explosive mixture exceeds in volume the olefiant gas, there is a more decided action upon the latter, manifested by an increased production of carbonic acid. Thus, for example, the explosive mixture being to the olefiant as $2\frac{1}{2}$ to 1,

about one-fourth of the olefiant gas was consumed; and by increasing the proportion of the explosive mixture, the olefiant gas was still more acted upon. On using oxygen sufficient to saturate both the hydrogen and the olefiant gases, the ball acted much more rapidly; in several instances it became red hot; all the hydrogen was consumed; and the whole of the olefiant gas was changed into water and carbonic acid. In this case the use of the sponge is inadmissible, as it kindles the gases, and occasions their detonation.

II. *Mixtures of Hydrogen and Carburetted Hydrogen Gases with Oxygen.*

When carburetted hydrogen, procured from stagnant water, was added to an explosive mixture, in various proportions between equal volumes, and ten of the former to one of the latter, the action of the hydrogen and oxygen on each other took place as usual, on admitting one of the balls. When, reversing the proportion, the explosive mixture was made to exceed the carburetted hydrogen, but not more than four or five times, the latter gas was entirely unchanged. With a larger proportion of the explosive mixture carbonic acid was always found to have been produced; but still the carburetted hydrogen was very imperfectly consumed, and fully three-fourths of it were generally found to have escaped unburned.

When, to a mixture of hydrogen and carburetted hydrogen, oxygen enough was added to saturate both gases, the effect of the sponge was found to vary with the proportion of the simple hydrogen. In several cases, where the hydrogen did not exceed the carburetted hydrogen more than four times, the latter gas remained unchanged; when in

larger proportion, there was a decided action upon the carburetted hydrogen. But it was much more easy to regulate the action of the balls upon such a mixture so as to act upon the hydrogen and oxygen only, than in the case of olefiant gas, which, under similar circumstances, is always more largely converted into water and carbonic acid.

III. *Mixtures of Hydrogen and Carbonic Oxide with Oxygen.*

The addition of one volume of carbonic oxide to two volumes of an explosive mixture produces a distinct effect in suspending the action of the platinum balls, and even of the spongy metal itself. The action of the gases upon each other still, however, goes on slowly, even when the carbonic oxide exceeds the explosive mixture in volume; and after the lapse of a few days, the oxygen is found to have disappeared, and to have partly formed water, and partly carbonic acid. I made numerous experiments to ascertain whether the oxygen, under these circumstances of slow combustion, is divided between the carbonic oxide and the hydrogen, in proportions corresponding to the volumes of those two gases. The combustible gases being in equal volumes, and the oxygen sufficient to saturate only one of them, it was found that the oxygen, which had united with the carbonic oxide, was to that which had combined with the hydrogen, as about 5 to 1 in volume. Increasing the carbonic oxide, a still larger proportion of oxygen was expended in forming carbonic acid. On the contrary, when the hydrogen was increased, a greater proportional quantity of oxygen went to the formation of water. But it was remarkable, that when the hydrogen was made to exceed the carbonic oxide four or five times, less

oxygen in the whole was consumed than before; the activity of the carbonic oxide appearing to have been diminished, without a corresponding increase in that of the hydrogen.

In cases, where the proportion of the carbonic oxide to the explosive mixture was intentionally so limited, that the platinum ball was capable of immediately acting upon the latter, the carbonic oxide was always in part changed into carbonic acid, the more abundantly as its volume was exceeded by that of the explosive mixture. Increasing the oxygen, so that it was adequate to saturate both gases, and causing the hydrogen to exceed the carbonic acid in volume, a speedy action was always exerted by the ball, and the whole of the combustible gases was silently converted into water and carbonic acid. The introduction of the platinum sponge into such a mixture was almost always found to produce detonation.

IV. *Mixtures of Hydrogen and Cyanogen with Oxygen.*

When one of the platinum balls, after being recently heated, is introduced into cyanogen and explosive mixture in equal volumes, no apparent action takes place. With half a volume of cyanogen there is a slight diminution; and as we reduce the proportion of that gas, the action of the elements of the explosive mixture on each other becomes more and more distinct. There is not, however, as with carbonic oxide, any production of carbonic acid; but in the course of a few minutes the inside of the tube becomes coated with a brownish substance, soluble in water, and communicating to it the same colour; having a smell resembling that of a burnt animal substance; and yielding ammonia on the addition of a drop or two of liquid potash. It was produced in too small a

quantity to enable me to submit it to a more minute examination; but its characters appeared to resemble those of a product, obtained by M. GAY LUSSAC, by mixing cyanogen with ammoniacal gas.*

If oxygen be added to a mixture of hydrogen and cyanogen, in quantity sufficient to saturate both the gases, it is still necessary, in order that an immediate effect should be produced by the sponge, that the hydrogen should exceed the cyanogen in volume. A decided action then takes place; an immediate absorption ensues; fumes of nitrous acid vapour appear, which act on the surface of the mercury; and, after removing the nitrous acid by a drop or two of water, and transfering the gas into a dry tube, carbonic acid is found to have been produced, equivalent in volume to double that of the cyanogen.

V. *Effect of adding various other Gases to an Explosive Mixture of Hydrogen and Oxygen.*

It had been already ascertained by Professor DOBEREINER, that one volume of oxygen, diluted with 99 volumes of nitrogen, is still sensible, when mixed with a due proportion of hydrogen, to the action of the sponge.† Carbonic acid, also, even I find when it exceeds the explosive mixture ten times, retards only in a slight degree the energy of the sponge. Oxygen, hydrogen, and nitrous oxide gases, when employed

* Annales de Chimie, XCV. 196.

† In analyzing atmospheric air by adding hydrogen to it, and acting on the mixture by a platinum ball, I have generally obtained a diminution indicating more than 21 per cent. of oxygen. This I find to be owing to the absorption of a small quantity of nitrogen by the ball, especially when, after being heated, it has been rapidly passed hot through the mercury.

to dilute an explosive mixture, are equally inefficient in preventing the mutual action of its ingredients. Ammonia may be added in ten times the volume of the explosive mixture, and muriatic acid gas in six times its volume, with no other effect than that of rendering the action of the sponge less speedy.

VI. *Mixtures of Carbonic Oxide and Carburetted Hydrogen with Oxygen.*

When mixtures of these gases are exposed to the sponge, the carburetted hydrogen seems to stand entirely neutral. The carbonic oxide is converted into carbonic acid, in the same gradual manner as if it had been mixed with oxygen only, and the carburetted hydrogen remains unaltered.

VII. *Mixtures of Hydrogen, Carburetted Hydrogen, and Carbonic Oxide with Oxygen.*

In mixtures of these gases, it is of little consequence whether the oxygen be sufficient for the hydrogen and carbonic oxide only, or be adequate to the saturation of all three. The circumstance, which has the greatest influence on the results of exposing such mixtures to the sponge, is the proportion which the simple hydrogen bears to the other gases, and especially to the carbonic oxide; for in order that there may be any immediate action, the former should exceed the latter in volume. In that case the hydrogen is converted into water, and the carbonic oxide into carbonic acid; but the carburetted hydrogen, unless the excess of hydrogen be very considerable, remains unaltered. If the proportion of hydrogen be so small, that no immediate action is excited by the sponge,

the ingredients of the mixture nevertheless act slowly upon each other; and after a few days, the whole of the hydrogen and carbonic oxide are found to have united with oxygen, and the carburetted hydrogen to remain of its original volume.

VIII. *Mixtures of Hydrogen, Carbonic Oxide, and Olefiant Gases with Oxygen.*

When the oxygen, in a mixture of these gases, is sufficient to saturate the two first only, and the proportion of hydrogen is so adjusted that the action of the sponge is not very energetic, the hydrogen and carbonic oxide only are acted upon; but if the diminution of volume, which the sponge produces, be rapid and considerable, part of the olefiant gas is converted into water and carbonic acid. This effect on olefiant gas takes place still more readily, if the oxygen present be adequate to the saturation of all three combustible gases.

It is remarkable, that if to a mixture of hydrogen, carbonic oxide, and oxygen, in such proportions that the sponge would act rapidly in producing combination, olefiant gas be added, the action of the gases on each other is suspended. Thus 20 measures of carbonic oxide, 31 of hydrogen, and 28 of oxygen, were instantly acted upon by the sponge; but the addition of 20 measures of olefiant gas to a similar mixture entirely suspended its efficiency. By standing fourteen days, rather more than half the carbonic oxide was acidified, and about one-twelfth of the hydrogen was changed into water, but the olefiant gas remained unaltered.

IX. *Mixtures of Hydrogen, Carbonic Oxide, Carburetted Hydrogen, and Olefiant Gases with Oxygen.*

In mixtures of these four gases with oxygen, it was found, by varying the proportion of hydrogen, that hydrogen and carbonic oxide are most easily acted upon ; then olefiant gas ; and carburetted hydrogen with the greatest difficulty. When the action of the sponge was moderate, only the hydrogen and carbonic oxide were consumed, or at most the olefiant gas was but partially acted upon. Adding more hydrogen, so as to occasion a more rapid diminution, the olefiant gas also was burned ; but the carburetted hydrogen always escaped combustion, unless the hydrogen were in such proportion that the ball or sponge became red hot.

From the facts which have been stated, it appears that when the compound combustible gases mixed with each other, with hydrogen, and with oxygen, are exposed to the platinum balls or sponge, the several gases are not acted upon with equal facility ; but that carbonic oxide is most disposed to unite with oxygen ; then olefiant gas ; and lastly, carburetted hydrogen. By due regulation of the proportion of hydrogen, it is possible to change the whole of the carbonic oxide into carbonic acid, without acting on the olefiant gas or carburetted hydrogen. With respect indeed to olefiant gas, this exclusion is attended with some difficulty, and it is generally more or less converted into carbonic acid and water. But it is easy, when olefiant gas is absent, so to regulate the proportion of hydrogen, that the carbonic oxide may be entirely acidified, and the whole of the carburetted hydrogen be left unaltered. This will generally be found to have been

accomplished, when the platinum ball has occasioned a diminution of the mixture, at about the same rate as atmospheric air is diminished by nitrous gas, when the former is admitted to the latter in a narrow tube.

SECTION II.

ON THE EFFECT OF FINELY DIVIDED PLATINUM ON GASEOUS MIXTURES AT INCREASED TEMPERATURES.

The effect of varying the proportion of free hydrogen to the compound combustible gases, on the degree of action which is excited by the platinum sponge, will perhaps admit of being explained, by examining the facts that have been stated, in connection with the degrees of combustibility of the compound gases under ordinary circumstances. The precise degree of temperature at which any one of them burns is not known, on account of the imperfection of our present methods of measuring high degrees of heat. It has been ascertained, however, by Sir HUMPHRY DAVY,* that at a heat between that of boiling mercury, and that which renders glass luminous in the dark, hydrogen and oxygen gases unite silently, and without any light being evolved; that carbonic oxide is as inflammable as hydrogen; that olefiant gas is fired by iron and charcoal heated to redness; but that carburetted hydrogen, to be inflamed, requires that the wire should be white hot. Now this is precisely the order in which the three compound gases require hydrogen to be added to them, in order to be rendered susceptible of being acted upon by the platinum sponge; carbonic oxide being acted upon

* On Flame, 8vo. p. 72.

with the smallest proportion of hydrogen; olefiant gas requiring more hydrogen, and carburetted hydrogen a still larger proportion. It is extremely probable, then, that the temperature, produced by the union of the hydrogen and oxygen forming part of any mixture, is the circumstance which determines the combustible gases to unite, or not, with oxygen by means of the sponge. It was desirable, however, to ascertain the exact temperature at which each of those three gases unites with oxygen with the intervention of the spongy platinum. For this purpose the gases, mixed with oxygen enough to saturate them, were severally exposed in small retorts containing a platinum sponge, and immersed in a mercurial bath, to a temperature which was gradually raised till the gases began to act on each other. In this way the following facts were determined.

1st. Carbonic oxide began to be converted into carbonic acid at a temperature between 300° and 310° FAHRENHEIT. By raising the temperature to 340°, and keeping it at that point for 10 or 15 minutes, the whole of the gas was acidified, the condensation of volume in the mixture being equivalent to the oxygen which had disappeared.

2dly. Olefiant gas, mixed with sufficient oxygen, and in contact with the sponge, showed a commencement of decomposition at 480° FAHRENHEIT, and was slowly but entirely changed into carbonic acid by a temperature not exceeding 520° FAHRENHEIT. M M. DULONG and THENARD* state the same change to take place at 300° cent.=572° FAHRENHEIT; but having repeated the experiment several times, I find no reason to deviate from the temperature which I have assigned.

* Ann. de Chim. et de Phys. XXIII. 443.

3rdly. Carburetted hydrogen, exposed under the same circumstances, was not in the least acted upon by a temperature of 555° FAHRENHEIT, the highest of which, by an ARGAND's lamp, I was able to raise the mercurial bath. This, however, must have been near the temperature required for combination; for on removing the retort from the mercurial bath, and applying a spirit lamp, at such a distance as not to make the retort red hot, a diminution of volume commenced, and continued till all the carburetted hydrogen was silently converted into water and carbonic acid.

4thly. Cyanogen, similarly treated, was not changed at a temperature of 555° FAHRENHEIT, and on applying the flame of a spirit lamp to the tube, it produced no action till the tube began to soften.

5thly. Muriatic acid gas, mixed with half its volume of oxygen, began to be acted upon at 250° FAHRENHEIT. Water was evidently formed; and the disengaged chlorine, acting upon the mercurial vapour in the tube, formed calomel, which was condensed, and coated its inner surface.

6thly. Ammonical gas, mixed with an equal volume of oxygen, showed a commencement of decomposition at 380° FAHRENHEIT. Water was also in this case distinctly generated; and at the close of the experiment, nothing remained in the tube but nitrogen and the redundant oxygen.

I proceeded, in the next place, to examine the agency of finely divided platinum at high temperatures, on those mixtures of gases, which are either not decomposed, or are slowly decomposed, at the temperature of the atmosphere.

When carbonic oxide and hydrogen gases, in equal volumes, mixed with oxygen sufficient to saturate only one of

them, were placed in contact with the sponge, and gradually heated in a mercurial bath, the mixture ceased to expand between 300° and 310° FAHRENHEIT, and soon began to diminish in volume. On raising the temperature to 340°, and keeping it some time at that point, no further diminution was at length perceptible. From the quantity of carbonic acid, remaining at the close of the experiment, it appeared that four-fifths of the oxygen had united with the carbonic oxide, and only one-fifth with the hydrogen. When four volumes of hydrogen, two of carbonic oxide, and one of oxygen, were similarly treated, the hydrogen, notwithstanding its greater proportional volume, was still found to have taken only one-fifth of the oxygen, while four-fifths had combined with the carbonic oxide. These facts show that at temperatures between 300° and 340° FAHRENHEIT, the affinity of carbonic oxide for oxygen is decidedly superior to that of hydrogen; as, from the experiments before described, appears to be the case, also, at common temperatures.

But a similar distribution of oxygen, between carbonic oxide and hydrogen, does not take place, when those three gases are fired together by the electric spark. This will appear from the following table, in which the three first columns show the quantities of gases that were fired, and the two last, the quantities of oxygen that were found to have united with the carbonic oxide and with the hydrogen.

	Before firing.			After firing	
	Measure of Carb. Oxide.	Measure of Hydrog.	Measure of Oxygen.	Oxygen to Carb. Oxide.	Oxygen to Hydrogen.
Exp. 1	40	40	20	6	14
2	40	20	20	12	8
3	20	40	20	5	15

When equal volumes of carbonic oxide and hydrogen gases, mixed with oxygen sufficient to saturate only one of them, were exposed in a glass tube to the flame of a spirit lamp, without the presence of the sponge, till the tube began to soften, the combination of the gases was effected without explosion, and was merely indicated by a diminution of volume, and an oscillatory motion of the mercury in the tube. At the close of the experiment, out of twenty volumes of oxygen, eight were found to have united with the carbonic oxide, and twelve with the hydrogen, proportions which do not materially differ from the results of the first experiment in the foregoing Table. At high temperature, then, the attraction of hydrogen for oxygen appears to exceed that of carbonic oxide for oxygen: at lower temperatures, especially when the gases are in contact with the platinum sponge, the reverse takes place, and the affinity of carbonic oxide for oxygen prevails.

Extending the comparison to the attraction of olefiant and hydrogen gases for oxygen at a red heat, I found that when six volumes of olefiant, six of hydrogen, and three of oxygen were heated by a spirit lamp till the tube softened, a silent combination took plate as before; all the oxygen was consumed; but only half a volume had been expended in forming carbonic acid, which indicates the decomposition of only one quarter of a volume of olefiant gas. On attempting a similar comparison between carbonic oxide and olefiant gas, by heating them with oxygen in the same proportions, the mixture exploded as soon as the glass became red hot, and burst the tube.

The property inherent in certain gases, of retarding the

action of the platinum sponge, when they are added to an explosive mixture of oxygen and hydrogen, is most remarkable in those which possess the strongest attraction for oxygen; and it is probably to the degree of this attraction, rather than to any agency arising out of their relations to caloric, that we are to ascribe the various powers which the gases manifest in this respect. This will appear from the following Table, the first column of which shows the number of volumes of each gas required to render one volume of an explosive mixture of hydrogen and oxygen uninflammable by the discharge of a Leyden jar; while the second column shows the number of volumes of each gas necessary, in some cases, to render one volume of an explosive mixture insensible to the action of the sponge, and in other cases indicates the number which may be added without preventing immediate combination. In the first column, the numbers marked with an asterisk were determined by Sir HUMPHRY DAVY; the remaining numbers in that column, and the whole of the second, are derived from my own experiments.

1 vol. of Explosive Mixture was rendered incapable of being inflamed by Electricity, when mixed with			Effect of adding the same Gasses to 1 vol. of Explosive Mixture on the action of the Sponge.
*	About 8 vol. of	Hydrogen	not prevented by many vols.
	6	Nitrogen	ditto.
*	9	Oxygen	not prevented by 10 vol.
*	11	Nitrous Oxide	ditto.
	1.5	Cyanogen	prevented by 1 vol.
*	1	Carbonized Hydrogen	not prevented by 10 vol.
	4	Carbonic Oxide	prevented by ½ a vol.
*	0.5	Olefiant Gas	prevented by 1.5 vol.
*	2	Muriatic Acid	not prevented by 6 vol.
	2	Ammonia	not prevented by 10 vol.
	3	Carbonic Acid	ditto.

From the foregoing Table it appears, that carbonic oxide produces the greatest effect, in the smallest proportion to an explosive mixture of oxygen and hydrogen, in preventing the action of those gases on each other, when exposed to the sponge at temperatures below the boiling point of mercury. In general, those gases which either do not unite with oxygen, or unite with it only at high temperatures, have little effect in restraining the efficiency of the sponge. There is an apparent exception, however, in cyanogen, which it would require more research than I have yet had time to devote to an object merely collateral to reconcile it, if it be capable of being reconciled, with the general principle.

From the fact that carbonic oxide, olefiant gas, and carburetted hydrogen, when brought to unite with oxygen by means of the platinum sponge assisted by heat, undergo this change at different temperatures, it seemed an obvious conclusion, that by exposing a mixture of those gases with each other and with oxygen to a regulated temperature, the correct analysis of such mixtures might probably be accomplished. Mixtures of two or more of the combustible gases were therefore exposed, in contact with oxygen gas and the platinum sponge, in tubes bent into the shape of retorts, which were immersed in a mercurial bath. This bath was gradually heated to the required temperatures, and by proper management of the source of heat, was prevented from rising above that degree.

1st. By subjecting 25 measures of carbonic oxide, 15 of olefiant gas, and 57 of oxygen, in contact with the sponge, to a heat which was not allowed to exceed 350° FAHRENHEIT till the diminution of volume ceased, all the carbonic oxide

was converted into carbonic acid, and the olefiant gas remained in its original volume.

2d. By exposing in a similar manner 20 measures of carbonic oxide, 21 of carburetted hydrogen, and 36 of oxygen, to a temperature below 400° FAHRENHEIT, the carbonic oxide was entirely acidified; and on washing out the carbonic acid by liquid potash, the carburetted hydrogen was found unaltered, mixed with the redundant oxygen.

3d. A mixture of 10 measures of olefiant gas, 10 of carburetted hydrogen, and 58 of oxygen, being heated in contact with the sponge to 510° FAHRENHEIT, the olefiant gas was silently but entirely changed into carbonic acid, while the carburetted hydrogen was not at all acted upon.

4th. By acting with the sponge upon 42 measures of carburetted hydrogen, 22 of carbonic oxide, 22 of hydrogen, and 28 of oxygen, first at a temperature of 340° FAHRENHEIT, which was raised gradually to 480°, all the carbonic oxide was changed into carbonic acid, and all the hydrogen into water; but the carburetted hydrogen remained undiminished in quantity, and was found, after removing the carbonic acid, mixed only with the redundant oxygen. In this experiment, the diminution of volume had continued some time before there was any perceptible formation of water, the attraction of carbonic oxide for oxygen appearing to prevail over that of hydrogen. The same precedency in the formation of carbonic acid is always apparent, when carbonic oxide and hydrogen, mixed even with oxygen enough to saturate both gases, are raised to 350° FAHRENHEIT.

By thus carefully regulating the temperature of the mercurial bath, the action of oxygen upon several gases, (carbonic

oxide, olefiant, and carburetted hydrogen for example) may be made to take place in succession; and by removing the carbonic acid, formed at each operation, it may be ascertained how much of each of the two first gases has been decomposed. The carburetted hydrogen indeed always remains unchanged, and its quantity must be determined by firing it with oxygen by the electric spark. If hydrogen also be present, it is difficult to prevent the olefiant gas from being partially acted upon; but this is of little consequence, as I had shown that it is easy to remove that gas in the first instance by chlorine.* It may be remarked, that this method of operating on the aëriform compounds of charcoal gives more accurate results than rapid combustion by the electric spark, being never attended with that precipitation of charcoal, which is often observed when the gases are exploded with oxygen. A regulated temperature, also, effects the analysis of such mixtures much more correctly than the action of the sponge or balls, because in the latter case the heat produced is uncertain; and though sometimes adequate to the effect, yet there is always a risk that it may exceed, or fall short of that degree, which is required for the successful result of the analytic process.

From the facts which have been stated, I derived a method of obtaining carburetted hydrogen gas perfectly free from olefiant gas, hydrogen, and carbonic oxide, and mixed only with a little oxygen, which, had it been necessary to my purpose, might also have been separated. The early product of the distillation of pit-coal was washed with a watery solution

* Philosophical Transactions, 1821, p. 147.

of chlorine, and afterwards with liquid potash, to remove a little chlorine that arose into the gas from the solution. The residuary gas was next heated with one-fourth its volume of oxygen, at the temperature of 350° FAHRENHEIT, in contact with the sponge; which converted the carbonic oxide into carbonic acid, and the hydrogen into water. The carbonic acid being removed by liquid potash, there remained only the carburetted hydrogen, the redundant oxygen, and a very minute quantity of nitrogen introduced by the latter gas. Hitherto, I have prepared this gas only in a small quantity, but it would be easy to extend the scale of the operation, and to remove the excess of oxygen by obvious methods.

SECTION III.

APPLICATION OF THE FACTS TO THE ANALYSIS OF MIXTURES OF THE COMBUSTIBLE GASES IN UNKNOWN PROPORTIONS.

At an early period of the investigation described in the first section, I proceeded to apply the facts of which I was then possessed, to the analysis of a mixture of gases in unknown proportions. For this purpose, I caused a quantity of gas to be collected from coal, by continuing the application of heat to the retorts two hours beyond the usual period, and receiving the gas into a separate vessel. Gas of this quality was purposely chosen, because, from former experience, I expected it to contain free hydrogen, carbonic oxide, and carburetted hydrogen, but no olefiant gas, the production of which is confined to the early stages of the progress. After washing it, therefore, with liquid potash to remove a little carbonic acid, and ascertaining its specific gravity when thus

washed to be 308, I proceeded at once to subject it to the new method of analysis.

Having ascertained, by a previous experiment with VOLTA's eudiometer, that 10 volumes of the gas required for saturation 9 volumes of oxygen, I mixed 43 measures with 43 of oxygen (= 41 pure) and passed a platinum ball, which had been recently heated, into the mixture. An immediate diminution of volume took place, attended with a production of heat, and formation of moisture. The residuary gas, cooled to the temperature of the atmosphere, measured 43.5 volumes. Of these 4.5 were absorbed by liquid potash, indicating 4.5 carbonic acid, equivalent to 4.5 carbonic oxide; the rest, being fired in a VOLTA's eudiometer with an additional quantity of oxygen, gave 11 volumes of carbonic acid; the diminution being 22 volumes, and the oxygen consumed 22 also, circumstances which prove that 11 volumes of carburetted hydrogen were consumed by this rapid combustion. But of the loss of volume first observed, (viz. $86 - 43.5 = 42.5$) 2.25 are due to the carbonic acid formed; and deducting this from 42.5, we have 40.25, which are due to the oxygen and hydrogen converted into water; and $40.25 \times \frac{2}{3} = 26.8$ shows the hydrogen in the original gas. But the sum of these numbers $(26.8 + 4.5 + 11)$ being less by 0.7 than the volume of gas submitted to analysis, we may safely consider that fraction of a measure to have been nitrogen. The composition then of the mixture will stand in volumes as follows:

Hydrogen . . .	26.8 . . .	62.32
Carbonic oxide . .	4.5 . . .	10.50
Carburetted hydrogen	11.0 . . .	25.56
Nitrogen	0.7 . . .	1.62
	43.0	100. 0

On calculating what should be the specific gravity of a mixture of gases in the above proportions, it was found to be .303,* which coincides, as nearly as can be expected, with the actual specific gravity of the gas submitted to analysis, *viz.* .308. To place the correctness of the results beyond question, I mingled the gases in the above proportions, and acted on the artificial mixture in the same manner as on the original gas, when I had the satisfaction to find that the analytical process again gave the true volumes with the most perfect correctness for the hydrogen and carbonic oxide, and within the fraction of a measure for the carburetted hydrogen. Notwithstanding this successful result, which was twice obtained, I should still prefer, for the reason which has been stated, having recourse to a temperature carefully regulated, for the analysis of similar mixtures, in all cases where the hydrogen is in moderate proportion, and where great accuracy is desirable. Whenever (it may again be remarked) olefiant gas is present in a mixture, it should always be removed by chlorine, before proceeding to expose the mixture to the agency of the spongy metal.

It can scarcely be necessary to enter into further details respecting methods of analysis, the application of which to particular cases must be sufficiently obvious, from the experiments which have been described on artificial mixtures. The apparatus required is extremely simple, consisting, when the balls are employed, of graduated tubes of a diameter between 0.3 and 0.6 of an inch; or, when an increased temperature is used, of tubes bent into the shape of retorts, of a diameter

* In this estimate, the specific gravity of hydrogen is taken at .0694; that of carbonic oxide at .6722; of carburetted hydrogen at .5555; and of nitrogen at .9728.

varying with the quantity of gas to be submitted to experiment, which may be from half a cubic inch to a cubic inch or more. These, when in use, may be immersed in a small iron cistern containing mercury, and provided with a cover in which are two holes, one for the tube, and the other for the stem of a thermometer, the degrees of which are best engraved on the glass.

By means of these improved modes of analysis, I have already obtained some interesting illustrations of the nature of the gases from coal and from oil. I reserve, however, the communication of them, till I have had an opportunity of pursuing the enquiry to a greater extent, and especially of satisfying myself respecting the exact nature of the compound of charcoal and hydrogen, discovered some years ago by Mr. DALTON, in oil gas and coal gas, which agrees with olefiant gas in being condensible by chlorine, but differs from it in affording more carbonic acid and consuming more oxygen.

Manchester, 6th June, 1824.

XVI. *A Comparison of Barometrical Measurement, with the Trigonometrical Determination of a Height at Spitzbergen. By Captain* EDWARD SABINE, *of the Royal Regiment of Artillery, F. R. S.*

Dated from Spitzbergen, July 24, 1823.

Read May 6, 1824.

THE hill selected for the comparative measurement was, as far as could be judged, the highest, within convenient distance, of which the ascent was practicable, being rather above the general height of the hills on the western part of the north coast of Spitzbergen; the summit was distant less than two miles from the Observatory on the Inner Norway Island, in a direction very nearly due south, as the mark, which was placed to determine the point of measurement, was within the field of the meridian transit instrument: the hill was situated on the main land, and was divided from the island on which the Observatory was established, by a sea channel of little more than a mile across, making part of the harbour of Fair-haven. The annexed sketch of the harbour and of the adjacent coast will be sufficient to point out the positions of the hill and of the Observatory, and is the more necessary, as the plan of Fair-haven, published in Captain PHIPPS's Voyage, (in which an endeavour might otherwise be made to trace them,) is so exceedingly inaccurate though purporting to be from actual survey, that after having been nearly three weeks on the spot, I am even more perplexed than on the

day of arrival, to assign in the plan, the island which is intended to represent the one on which the Observatory is placed, or the position of the hill in question; the latter, I apprehend must have been designed either by the one marked *(a)* in Captain PHIPPS'S, (or rather in Mr. D'AUVERGNE'S) plan, or by that marked *f*, although neither corresponds, even within ordinary limits, in height, or in relative position. The present sketch, Plate XIII. is taken principally from a manuscript survey of Captain BEECHEY'S, when at Spitzbergen as a Lieutenant in Captain BUCHAN'S expedition of 1818; Captain BEECHEY'S Survey has been found remarkably correct wheresoever we have had an opportunity of verifying it.

The shore of the main land to the north eastward of the hill forms a small bay, which being frozen over, afforded a perfectly level base, in which no correction was required for inequalities of surface, and the consequent liability to error introduced in the reduction was avoided. Having stationed a line of poles in such manner as to cover each other exactly, by means of a telescope placed at the one extremity, the distance between the extremes was carefully measured with a GUNTER'S chain, by Mr. HENRY FOSTER, of His Majesty's Ship Griper, and myself, and was found to amount to 36 lengths, or 2376 feet; the chain was drawn along the surface of the ice at each remove, so that the links were prevented from entanglement; it was stretched at each repetition as tightly as two persons could draw against each other, and the spots marked by flat plates of iron, furnished with long spikes by which they were fixed securely in the ice; the temperature of the air was 35°, and of the chain 32°. In

a second measurement, with the same precautions as on the first occasion, the difference did not amount to more than an inch and half. The extremities of the base, being abreast of two projecting points of land, one on the main shore, and the other on a small rocky island, offsets were made at right angles to the base, each of 38 feet, and the spots carefully marked, as containing between them the distance originally measured, with the additional advantage of a firm foundation at the extremities for future operations. This base is the line marked A B in the annexed plan, Plate XIV.

A polished copper cone borne at the end of a staff was securely fixed at the summit of the hill; the apex of the cone was proposed as the height to be measured, and was 44 inches above the highest pinnacle of the hill.

The base had been chosen for convenience in measurement, although its direction was not the most suitable for obtaining the horizontal distance of the cone nor indeed was the cone itself visible from B; a third station, C, was therefore selected across the bay, close to the waters' edge, and the distance A C obtained from the base AB, and the angles at A, B, and C. A C thus became a base for the determination of the horizontal distance of the cone, enabling its height to be ascertained from its zenith distance observed at A and C.

The instrument used for the horizontal and vertical angles was a repeating circle upon the recently improved English construction; concerning which, as it regards the vertical angles, it will be sufficient to notice, that of six observations of the meridian zenith distance of the sun to obtain the latitude of the Observatory, whereof four were on

the northern and two on the southern meridian, the extreme differences in the latitude deduced from the results did not amount to 7 seconds; and as each zenith distance in the present determination is a mean of several repetitions, they may be presumed to be free from any error which could affect the conclusion. It was not considered necessary to go through the process of repetition in the horizontal angles, especially as there were four verniers on the circle; each angle, however, is the mean of separate observations by Mr. FOSTER and myself, both of which are inserted. It will be seen, that in the triangle A. B. C. the three observed angles fell short by 57 seconds of 180°. It was not however deemed necessary to repeat the observations, for the purpose of detecting an error which does not make a difference of one inch in the length AC, nor, consequently, in the altitude of the hill; a third of 57 seconds, or 19 seconds, has however been added to each of the observed angles to complete their sum to 180°.

A corroboration of the measurements from A and C was obtained by including the Observatory, which was visible from the three stations, and from whence the zenith distance of the cone was also observed; the angles were taken, as nearly as could be judged, to the middle of the door of the Observatory, which faced the south; as however it did not present so definite an object as a station pole, and as moreover the distance of the cone from the Observatory was much greater than from A and C, and the angle of elevation consequently much less, it may be preferable as a fixed determination to take a mean of the results at A and C, and to consider the one obtained at the Observatory simply as corroborative.

The following detail comprises the observations, and the computed results.

From Station A,

The circle being nine feet above half tide.

Horizontal Angles.	CAB.	FOSTER	63° 09′ 16″;	SABINE	63° 09′ 16″;	Mean	63° 09′ 35″.*
	OAB.	——	128 27 12,5;	——	128 27 17;	——	128 27 15.
	OAD.	——	124 08 35;	——	124 08 33;	——	124 08 34.
	OAC.	——	65 17 40;	——	65 17 40;	——	65 17 40.
Zenith distance of the Cone.	Mean of 4 observations . .			——	76 53 48		
	Mean of 2 observations . .			——	76 53 47	——	76 53 48.
	Mean of 2 observations . .			——	76 53 48		

From Station B.

Horizontal Angle A.B.C. FOSTER 57°40′11″,5; SABINE 57°40′15″. Mean 57°40′32″ *

From Station C,

The circle being nine feet above half tide.

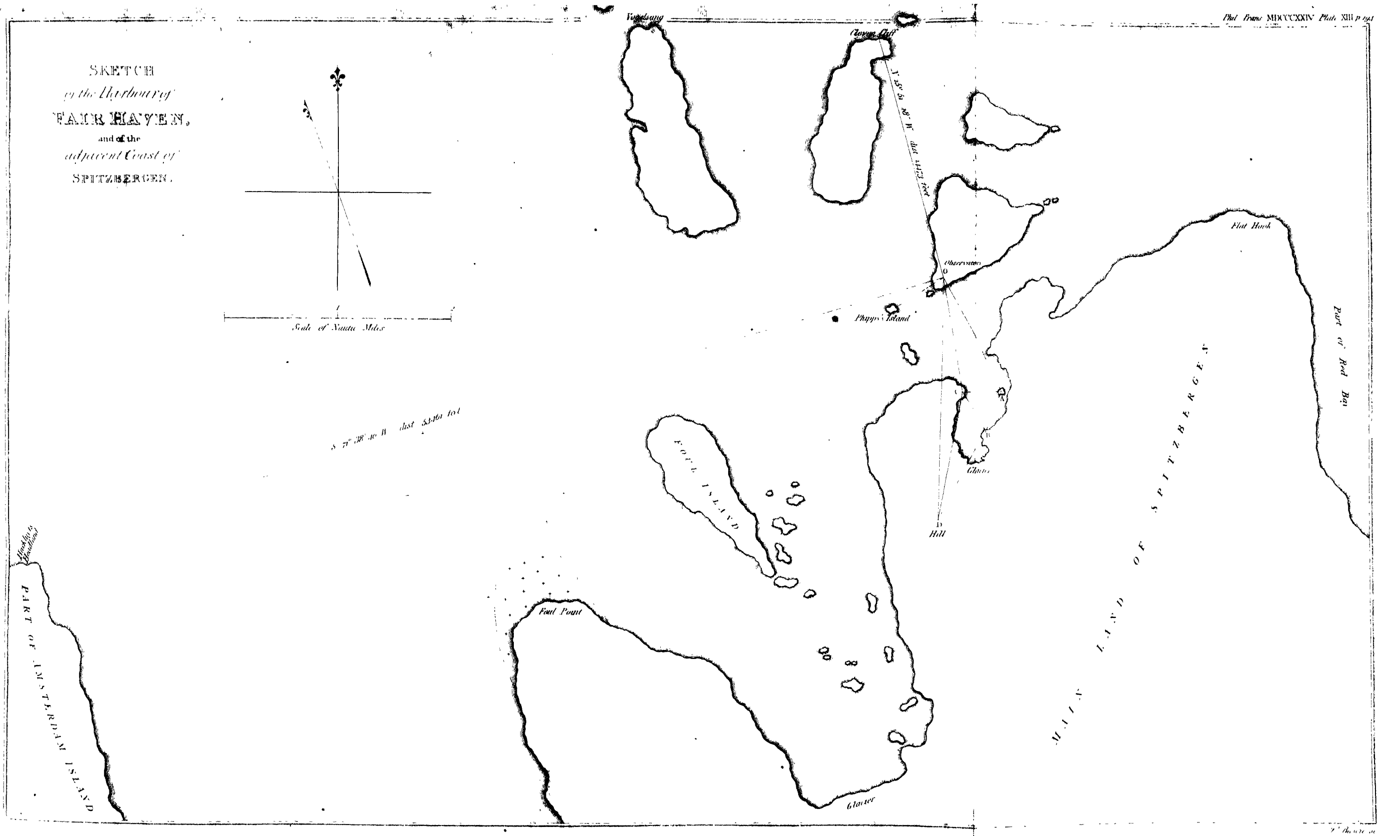
Horizontal Angles.	ACB.	FOSTER	59° 09′ 14″;	SABINE	59° 09′ 52″,5;	Mean	59° 09′ 52″.*
	ACD.	——	102 10 33;	.	. . . .	.	102 10 33.
	ACO.	——	96 40 36;	——	96 40 20;	——	96 40 28.
Zenith distance of the Cone.	Mean of 4 observations . .			——	75 07 45		
	Mean of 2 observations . .			——	75 07 46	. .	75 07 47
	Mean of 2 observations . .			——	75 07 50		

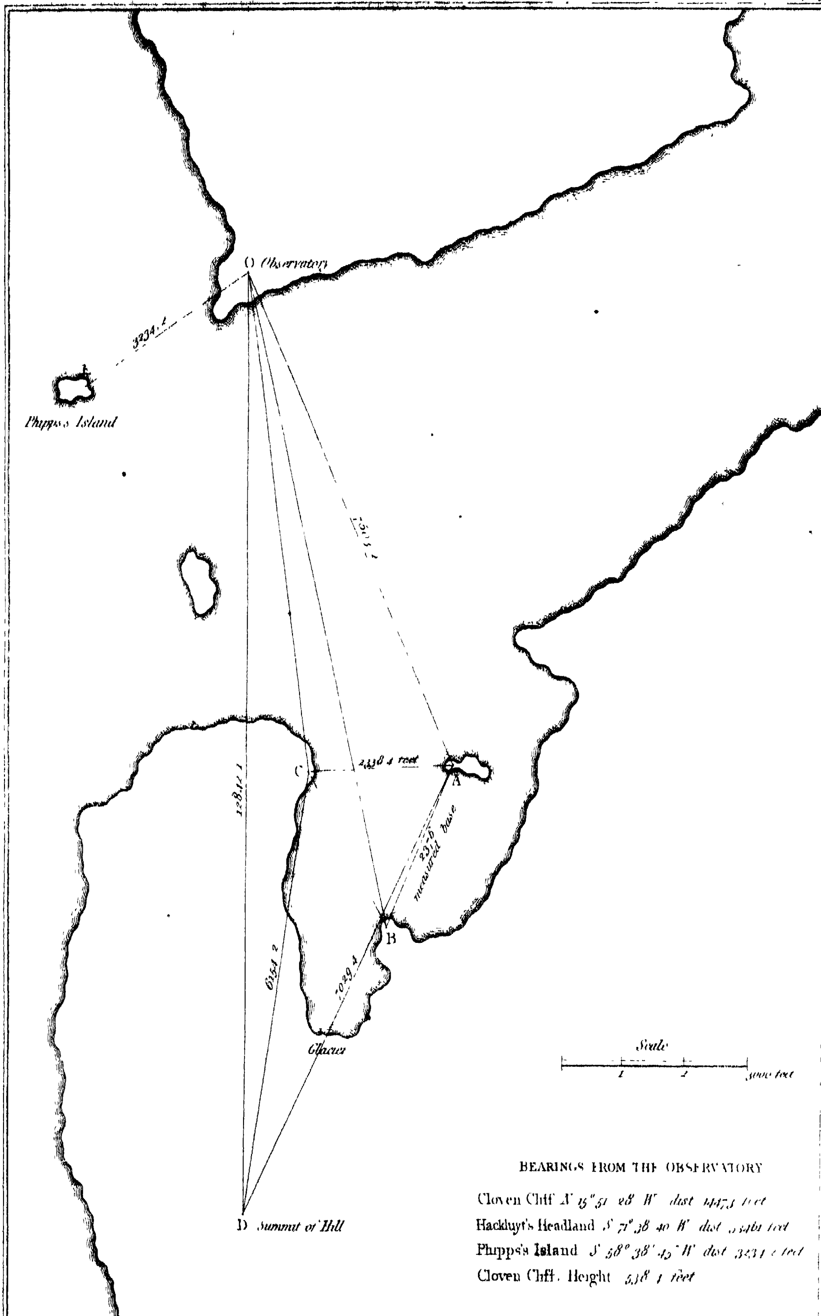
From the Observatory,

The circle being 31.5 *feet above half tide.*

Zenith distance of the Cone.	Mean of 4 observations;	SABINE	82°51′15″	
	Mean of 2 observations;	——	82 51 20.	. . 82° 51′ 17″,5.

* One third of 57″ added to complete the sum of the angles at A, B, and C to 180°.


SKETCH
of the Harbour of
FAIR HAVEN,
and of the
adjacent Coast of
SPITZBERGEN.
Scale of Nautic Miles
S 71° 38' 40" W dist 53460 feet
N 15° 51' 18" W dist 14173 feet
Vogelsang
Cloven Cliff
Observatory
Phipps's Island
Hill
Glacier
Flat Hook
Part of Red Bay
MAIN LAND OF SPITZBERGEN
FOUL ISLAND
Foul Point
Glacier
Hackluyts Headland
PART OF AMSTERDAM ISLAND



BEARINGS FROM THE OBSERVATORY

Cloven Cliff *N* 15° 51′ 28″ *W* *dist* 14473 *feet*
Hackluyt's Headland *S* 71° 38′ 40″ *W* *dist* 33461 *feet*
Phipps's Island *S* 58° 38′ 45″ *W* *dist* 3234.2 *feet*
Cloven Cliff. Height 538.1 *feet*

Deductions.

AC = 2338,35 feet. AO = 7503,2 feet.
AD = 7029,4 feet. OD = 12842,1 feet.
CD = 6154,2 feet.

Altitude of the Cone.

From Station A.	*From Station C.*	*From the Observatory.*
1645,2 feet.	1643,1 feet.	1641,4 feet.

The mean of the three results is 1643,2 feet; and of the first and second 1644,15 feet; and if one-twelfth of the intercepted arc be added to the zenith distance, to compensate for the effects of terrestrial refraction, the first mean will be reduced to 1642,8 feet, and the second, which is considered the preferable, for reasons already assigned, to 1644 feet.

Barometrical Measurement.

The instruments employed for this purpose were a marine barometer made by Mr. Jones, and a mountain barometer made by Mr. Newman; the first having a glass cistern, and the second an iron cistern: Mr. John Frederic Daniell, Fellow of the Royal Society, whose attention had been particularly devoted to the construction and improvement of meteorological instruments, was kind enough, at my request, to superintend their progress through the makers' hands.

In the construction of these barometers, the tube, cistern, and scale, were secured, in the first instance, or designed to be secured, immoveably to each other, and the scale was subsequently graduated from the surface of the mercury in the cistern. It happened that this operation was performed in

both cases, when the height of the mercury in the tube was 30,400 inches, which thus became in both barometers a neutral point at which no correction was needed, equivalent to the more ordinary adjustment of the zero of the scale to the level of the cistern; and from whence, at all other heights of the mercury, a correction would obtain, additive to the reading if the height should exceed 30,400 inches, and subtractive if below that amount: the diameters of the tubes were respectively .31 and .15 inches; and their ratio to the capacity of the cisterns being of JONES'S as 1 to 11, and of NEWMAN'S as 1 to 54, $\frac{1}{11}$, and $\frac{1}{54}$ of the respective differences between the height of the mercury indicated by the scale, and 30,400 inches were the corrections to be applied.

The temperature of the mercury at the time of observation was shown in both the barometers by a thermometer having its bulb in the cistern and immersed in the mercury itself; the verniers were furnished with a tangent screw for slow motion, and being made to encompass the tube, assured the proper position of the eye in adjusting the zero of the vernier to the surface of the mercury; the adjustment was capable of being made with much precision by the assistance of a microscope.

The instruments having been thus constructed independently of each other, and without reference to other barometers, were compared at Mr. NEWMAN'S house, on their completion in May 1823, by Mr. DANIELL, Mr. NEWMAN, Mr. JONES, and myself, as follows:—

	A.M.		P.M.	
	Newman's.	Jones's.	Newman's.	Jones's.
Mr. Daniell .	30.188	30.192	30.110	30.120
Mr. Newman .	30.188	30.200	30.106	30.119
Mr. Jones . .	30.182	30.192	30.116	30.120
Captain Sabine	30.180	30.191	30.103	30.122
Mean . . .	30.1845	30.1937	30.1087	30.1202
Capacity . . .	—.0039	—.0187	—.0052	—.0254
Capillary action	+.0880	+.0280	+.0880	+.0280
	30.2686	30.2030	30.1915	30.1228
Differences; Jones's less	.0656.		.0687.	
	Mean .067			

The cause of the difference of .067 parts of an inch in the indications of the two barometers did not appear obvious, either to Mr. Newman or to Mr. Jones; and though its occurrence was not quite so satisfactory as a perfect, or even as a nearer accordance might have been, it was no otherwise important than as it made an index correction of .067 necessary to be applied, additive to Mr. Jones's or subtractive to Mr. Newman's, to cause the height of the one to be inferred from that of the other.

On my arrival in Norway in June, I was surprised and mortified to perceive, that, after so much pains had been bestowed, a greater difference obtained in the indications of the barometers than in the former comparison; on a close examination, a small quantity of mercury was discovered to have lodged between the cistern of Mr. Jones's and its brass enclosure, which I at first concluded had escaped from the cistern; no leak was however perceptible, and the cistern and tube, as well as every other part of the barometer were

apparently uninjured. I had with me a third barometer made by Mr. NEWMAN for a Gentleman at Hammerfest, at the same time and with equal care as the others, and which had been compared with them in London: by means of the third barometer the alteration was ascertained to have taken place in Mr. JONES'S, and not in either of Mr. NEWMAN'S. It has since occurred to me, as affording a possible explanation of the alteration, that after the comparison in London, Mr. JONES took his barometer with him to his house to engrave the neutral point, and the diameter of the tube, upon the brass cover of the cistern, and I remember that he spoke of one or two screws which required to be tightened, and which, he observed, might affect the relation of the cistern and scale. That the alteration should have taken place at that time and in some such manner; and that the mercury which appeared between the cistern and its cover had lodged there accidentally in the original putting together of the instrument, and that it was not therefore concerned in producing the change, would seem the more probable supposition, than that it should have escaped subsequently from the cistern; since, if a part of the mercury had found a means of escape, more would have followed either then or since, which however has not been so, as no alteration has taken place subsequently in the relative indications of the two barometers. Having ascertained this last fact on my arrival at Spitzbergen, in July, a careful series of comparisons was instituted to determine the exact amount of the alteration since the original comparison at Mr. NEWMAN'S, and to obtain a fresh index correction and neutral point, since each would be alike affected, whether the cause were a

displacement of the scale, or a diminution of mercury in the cistern. The subjoined comparisons will show that an alteration, amounting to 0.129 parts of an inch, being supposed to have taken place since the first comparison, making the index correction of Mr. JONES's + 0.196, and its neutral point 30.271, will reconcile all the comparisons within such limits as may readily be allowed as errors of observation. The mean difference of fourteen comparisons thus reduced is less than four ten-thousandths of an inch; whence it is reasonable to conclude, that from the same number of observations, made under similar circumstances, the height of NEWMAN's barometer may be inferred from that of JONES's, and *vice versa*, to an equal degree of exactness.

The table of comparisons shows the actual readings of the two barometers, corrected only for the expansion of mercury and of the scale to a mean temperature of 32°; the last column exhibits the differences in excess or defect, of JONES's on NEWMAN s, after the application of their respective correction for capacity, capillary action, and an index error of + 0.196.

	NEWMAN's.	JONES's.	*Diff.*
Hammerfest, June 10,	30.116	30.002	+.003
Spitzbergen, July 12,	29.890	29.791	+.003
- - 13,	29.867	29.764	—.003
- - 13,	29.857	29.757	—.001
- - 13,	29.864	29.762	—.002
- - 13,	29.860	29.765	+.005
- - 14,	29.888	29.783	—.003
- - 14,	29.910	29.798	—.010
- - 15,	29.937	29.826	—.006
- - 15,	29.885	29.784	000
- - 16,	29.811	29.715	000
- - 16,	29.756	29.666	+.003
- - 17,	29.698	29.611	+.002
- - 17,	29.721	29.633	+.003

Mean difference, JONES's in defect .0004 nearly

In the experiments for determining the height of the hill, NEWMAN'S barometer was employed on the summit, and JONES'S by the sea. The station of the lower barometer was on the outside of a house framed of wood with boarded walls, which was constructed for my pendulum experiments; the house was 12 feet square, and stood withinside a tent of sufficient dimensions to admit of passage room between the walls of the tent and house; the barometer was so suspended as to be entirely detached from the side of the house, and the walls of the tent were unhooked during the observations; the cistern was 21 feet above the level of half tide. NEWMAN'S barometer was suspended beneath the cone on the summit of the hill on the 17th of July, and was not removed from thence until the afternoon of the 21st, being suffered to remain under a temporary protection, which was of course removed during the observations; the cistern was on a level with the highest point of the hill, and 44 inches below the apex of the cone, which, being the proposed point of measurement, renders an addition of $21 + 3.66 = 24.66$ feet necessary to the result obtained by the barometric difference, to give the total height of the cone above half tide. The times at which the observations were repeated, were previously concerted; and as the motions of the observer on the summit of the hill were visible with a good telescope from the Observatory, the simultaneous observation was assured. The hygrometer and detached thermometer were used in all cases in the open air and in the shade: the hygrometer was the recent invention of Mr. DANIELL, which is distinguished by his name; the results deducible from the barometric differences, under the observed circumstances of the atmo-

sphere in respect to temperature and aqueous vapour, are computed agreably to the method published by Mr. Daniell, in the 18th volume of the Journal of the Royal Institution.

July 17*th. P. M.* The weather dull with little wind; the hills generally enveloped in fog.

	Observatory, Sabine.				Hill, Foster.			
App. time.	*Barom.*	*Merc.*	*Air.*	*Point of Deposition.*	*Barom.*	*Merc.*	*Air.*	*Point of Deposition.*
4.30	29.671	40	35	34	28.012	39	37.3	In a cloud more or less dense during the whole period.
5.00	29.673	40	35	34	28.009	36.2	35.2	
5.30	29.674	39.5	35	34	28.009	35.5	34.5	
6.00	29.676	39.5	34.5	34	28.000	34.9	34.9	
	29.6735	39.75	34.9	34	28.0075	36.4	35.5	35.5
Corrections: Reduction to 32°	—.0200				—.0105			
Capacity	—.0561				—.0445			
Capillary action	+.0280				+.0880			
Index	+.1960				. . .			
True Barom. heights	29.8214				28.0405			

Result. Height of the Cone 1644,58 *feet.*

July 18, *P. M.* A thick fog with a moderate breeze.

	Observatory, Sabine.				Hill, Foster.			
App. time.	*Barom.*	*Merc.*	*Air.*	*Point of Deposition.*	*Barom.*	*Merc.*	*Air.*	*Point of Deposition.*
3.20	29.675	40.5	36	Fog.	28.021	36.	33.5	Dense fog.
3.30	29.6755	40.5	36	-	28.021	35.5	34	-
4.00	29.677	40	35.5	-	28.014	36	35.5	-
4.30	29.678	39.2	35.6	-	28.024	36	34.8	-
5.00	24.679	39	35.2	-	28.033	35.8	35.8	-
5.30	29.681	39	35.2	-	28.026	34.8	34.8	-
6.00	29.682	39	35.2	-	28.029	34.5	33.5	-
6.30	29.684	39	35.2	-	28.033	35	34	-
7.00	29.685	39	35.2	-	28.029	34.5	34	-
7.30	29.686	39	35.2	-	28.027	35	33.5	-
8.00	29.686	39	35.2	-	28.029	34.5	33.	-
	29.6808	39.4	35.4	35.4	28.026	35.2	34.2	34.2
Corrections: Reduction to 32°	—.0190				—.0075			
Capacity	—.0554				—.0441			
Capillary action	+.0280				+.0880			
Index	+.1960				. . .			
True Barom. heights	29.8304				28.0624			

Result. Height of the Cone 1630,66 *feet.*

July 19-20. After midnight. Weather clear and fine.

	Observatory, Sabine.				Hill, Foster.			
App. time.	*Barom.*	*Merc.*	*Air.*	*Point of Deposition.*	*Barom.*	*Merc.*	*Air.*	*Point of Deposition.*
12.30	29.679	39.4	35	34.5	. . .	. . .	. . .	. . .
12.35	. . .	. . .	. . .	. . .	28.054	42	38.5	33.5
12.45	. . .	. . .	. . .	. . .	28.041	42.8	39.5	34.5
12.50	29.673	39.5	36	34.5	. . .	. . .	. . .	. . .
13.05	. . .	. . .	. . .	. . .	28.024	43.5	39.8	35
13.10	29.667	40	37	34.7	. . .	. . .	. . .	. . .
13.25	. . .	. . .	. . .	. . .	28.036	44.5	40	35.5
13.30	29.659	39.8	35.7	35.2	. . .	. . .	. . .	. . .
13.45	. . .	. . .	. . .	. . .	28.021	45	40.8	36.5
13.50	29.652	39.8	37	35.3	. . .	. . .	. . .	. . .
14.05	. . .	. . .	. . .	. . .	28.037	44.9	40.8	35.8
14.10	29.649	39.2	37.6	36	. . .	. . .	. . .	. . .
	29.6932	39.6	36.4	35	28.0355	43.8	39.9	35.1
Corrections: Reduction to 32°	—.0197				—.0281			
Capacity . . .	—.0570				—.0443			
Capillary action .	+.0280				+.0880			
Index . . .	+.1960				. . .			
True Barom. heights	29.8108				28.0511.			

Result. Height of the Cone 1635.4 *feet.*

July 21. A.M. Calm and clouded on the Hill; light rain at the Observatory.

	Observatory, Foster.				Hill, Sabine.			
App. time.	*Barom.*	*Merc.*	*Air.*	*Point of Deposition.*	*Barom.*	*Merc.*	*Air.*	*Point of Deposition.*
10.30	29.328	41.8	38.5	Rain	27.673	39.8	39.	39 Rain.
11.	29.325	41.9	39.2	—	27.666	38.8	38.2	36.5
11.30	29.323	42	40	—	27.663	39.2	39.	36.2
12.	29.319	42.2	40	—	27.660	40	38.8	37
12.30	29.317	42	41	—	27.661	41.3	39.8	37.2
	29.3224	42	39.7	39.7	27.6646	39.8	39	37.2
Reduction to 32° .	—.0248				—.0186			
Capacity . . .	—.0885				—.0510			
Capillary action .	+.0280				+.0880			
Index	+.1960 or +.1845				- - -			
True Barom. heights	29.4331 or 29.4216				27.683.			

Result. Height of the Cone { *Index Corr.* +.196 = 1652.06 *feet.*
Index Corr. +.1845 = 1641.7 *feet.* }

Having brought down NEWMAN's barometer from the hill to the Observatory on the afternoon of the 21st, its direct comparison with JONES's was resumed, for the purpose of ascertaining that it had sustained no injury, and to make an additional trial of the index correction and neutral point, as the height of the mercury was then much lower than in the former comparisons. The barometer had been progressively falling from the afternoon of the 19th, and continued to descend until between 5 A. M. and $7^h\ 30^m$ A. M. on the 22nd, when it reached 29.244 its lowest depression: it had fallen considerably, therefore, and was still falling when I began to compare the barometers on the afternoon of the 21st: the surface of the mercury in the tube of NEWMAN's, the diameter of which is only .15 parts of an inch, was convex as usual, though not so much so as when rising or stationary; but in JONES's, the diameter of which is double that of the other, or .31, the convexity had entirely disappeared, so that the zero of the vernier, when marking the level of the highest part of the mercury, coincided with the part which was in contact with the glass, and I was even doubtful whether there was not a slight concavity in the centre. I had not leisure at the time to examine the comparisons, but continued to repeat them at intervals, until the following morning, when, as the mercury began to rise, I became more strongly impressed than on the preceding evening, that it had been previously slightly depressed in the centre below the level of the sides, and that the depression had ceased to obtain. On applying the respective corrections to the comparative readings contained in the subjoined table, it appeared that as soon as the mercury had began to rise, the index correction of +.196

produced its former correspondence between the barometers, but that previously to that period, and when the mercury has been supposed to have been, as described, slightly concave in the tube of JONES'S, its indications with the additive of +.196, would be uniformly too high for the agreement; as much so on the average as .0105, or one-hundredth of an inch nearly. I have considered it preferable, therefore, to employ +.1845 as the index correction on the 21st instant, instead of +.196, as it was the difference actually observed by direct comparison on the same afternoon, and, as far as can be judged, under similar circumstances: the result, however, has been computed on both suppositions, and is inserted. No such uncertainty exists in any of the previous observations, as, on all the former occasions, the mercury in both barometers presented a more or less convex surface.

In the following table the actual readings are given as before, reduced to the temperature of 32° for the expansion of mercury, and the scale; and the last column shows the index correction required in each comparison to produce an agreement.

		NEWMAN'S.	JONES'S.	Index correction.	
July 21.	6 P.M.	29.311	29.259	.184	.1845
—	10 P.M.	29.314	29.262	.184	
—	12 P.M.	29.312	29.259	.186	
22.	3 A.M.	29.306	29.253	.186	
—	3.50 A.M.	29.303	29.253	.183	
—	5.00 A.M.	29.294	29.244	.183	
—	7.30 A.M.	29.303	29.250	.186	
—	12 Noon.	29.360	29.295	.195	.195
—	1.30 P.M.	29.367	29.302	.194	
—	2.30 P.M.	29.373	29.310	.194	
(FOSTER obs.) —	2.45 P.M.	29.383	29.316	.197	

The results of the barometrical measurements, collected in one view, are as follows:

July 17.	Height of the Cone.	1644.58 feet.		
July 18.	- - - -	1630.66		
July 19.	- - - -	1635.4		
July 21.	- - - -	1641.7	or with + .196	1652.06 feet.
		Mean 1638.08		1640.07

It may be seen that the result obtained on the 18th of July deviates more considerably than any of the others from the general mean; which may be in some measure, if not entirely, accounted for by a circumstance which was noticed by Mr. FOSTER at the time of registering the observations, namely, that the freshness of the wind on the hill obliged him to steady the barometer either with the hand or with a guy; the very slight deviation which may have been occasioned thereby from the perpendicular suspension of the instrument, would cause the ascent of the mercury in the tube, and in consequence render the deduction of the height of the cone erroneous in defect; and such it would appear to have been. It will be further seen that the particular observations on the hill varied more from each other on that day than on the other occasions: omitting the observations of the 18th of July, the mean barometrical result is 1640.5 feet.

The following table exhibits a general view of the several determinations;

Trigonometrical	From Station A	Altitude of the Cone.	1645 feet.
	From Station C	- - -	1642.9
	From the Observatory	- -	1640.6
Barometrical	July 17.	- - - - - -	1644.6
	July 18.	- - - - - -	1630.7
	July 19.	- - - - - -	1635.4
	July 21.	- - - - - -	1641.7

Whence, it may be concluded that in these experiments the two methods of measurement have been found to correspond within the limits, which, under circumstances, may be attributed to accidental, and indeed to unavoidable errors.

It may be expected that whilst on the spot, some means may have occurred to me of explaining the very great difference which is recorded by Captain PHIPPS to have been found in the height of an hill in Amsterdam island, measured geometrically by himself, and barometrically by Dr. IRVING, in which the latter measurement exceeded the former by 85 feet, in between fifteen and sixteen hundred feet; the observations on that occasion were conducted with so much apparent care, and the difference was so great, as to have caused more or less doubt to have prevailed from that time to the present, of the equal applicability of the barometric formula in the higher, as in the middle and lower latitudes. I do not however feel better able than before, to conjecture in which of the operations the mistake originated, for such I do not doubt there must have been. I was desirous to have repeated the measurement of the hill itself, but time did not permit; judging, however, by the eye, in comparison with other hills on the coast, and especially with the one which has been the subject of this communication, and which was not more than a few miles distant, the lowest, that is the geometric result, appeared the most likely to have been correct; nevertheless a mistake of nearly a tenth of an inch, is a great amount with a barometer which was registered to thousandths; especially as the tendency of probable errors is on the other side. For

example, it is very probable that the mercury may have been heated by the warmth of the person who carried it up the hill, which heat it would not part with so readily as the thermometer suspended by its side: I believe this to be a very frequent source of error in barometrical measurements, and that the insertion of the attached thermometer in the mercury itself in the cistern, is a great practical improvement. This error would have most effect, when the difference was greatest between the temperature of the air and that of the human body; but it would render the computed height less than the correct, whereas Dr. IRVING's measurement is already in excess. So also, as the barometer does not seem to have been furnished with a means of adjusting the scale to the level of the cistern, and as no correction appears to have been made for the descent of an inch of mercury from the tube into the cistern, its true height may have been, from this cause, actually less than the observed; but this also would encrease the elevation of the hill.

I had supposed it possible that the view of the summit, which was the station of the barometer, might have been intercepted from the low ground on which the base was measured, and from whence the angles of elevation were taken; but this conjecture was not borne out on the spot. I must leave it, therefore, in its former uncertainty, though I trust with this difference, that the question is no longer of the same interest or importance as before.

Having thus detailed the particulars of this comparative measurement, I may be allowed to notice, that I have had much disappointment in not having it in my power to try the experiment on a hill of greater elevation; but those

which exceeded it in height, were at such a distance in the interior, in a country so more than ordinarily difficult to traverse, that it would have required far more time than was at my disposal to have made the attempt. We were ourselves misled in our expectations, and were it not pointed out, others might still be so in their judgments, by the incorrectness with which the height of the hills on the Northern part of the coast of Spitzbergen are set down in the 8th plate of Captain PHIPPS'S voyage; and with all the appearance of the utmost accuracy. I have already expressed the belief that the hill marked *f* in that plan, was designed to represent the one now measured, in which case its inserted height, 2400 feet, is nearly ⅓rd, or 800 feet too high; and if it be not the same hill, it is still more in error. As its distance did not much exceed 1½ miles from the island where Captain PHIPPS'S base was measured, it is far more probable that the error has taken place in the insertion in the plan, rather than in the actual measurement, which was doubtless made with the same scrupulous attention to accuracy, with which Captain PHIPPS, and the scientific gentlemen who accompanied him, appear to have conducted other operations of the same kind; the genuine record might now have furnished materials, interesting perhaps in a geological view, of tracing how much, or possibly how little diminution in height, the naked and pointed summits of the Spitzbergen hills have sustained in the lapse of half a century, and in a climate which is considered as peculiarly destructive.

Spitzbergen, July 24, 1823.

Note. London, 1824. During a residence at Drontheim, in Norway, in the autumn of 1823, I had occasion to employ the same two barometers in measurements of heights, and I made, in consequence, the following comparisons of their respective indications, at times when the height of the mercury was very different: the results are in remarkable correspondences with those at Spitzbergen, in page 299, and may be considered an additional justification of the employment of the two index corrections on that occasion.

Newman's.	Jones's.	Index correction.
29.130	29.091	+ .183
30.254	30.128	+ .196

XVII. *Experimental Inquiries relative to the distribution and changes of the Magnetic Intensity in ships of war.* *By* George Harvey, *Esq.* *Communicated by* John Barrow, *Esq.* *F. R. S.*

Read Feb. 26, 1824.

It having appeared from many unquestionable experiments, that the variation of this compass, as determined on shipboard, is subject to remarkable anomalies, arising from the unequal influence of the iron distributed through the various parts of a vessel, and from the changeable intensity of the same, occasioned by the different directions of the ship's head, with respect to the magnetic meridian, and from its different situations on the surface of the earth, it seemed desirable that some attempt should be made, to discover in what way the attractive forces are distributed throughout the vessel, and particularly in the vicinity of the binnacle, by a series of careful experiments.

To trace the variations in the intensity of the magnetic forces, under the simplest circumstances possible, the Scylla gun brig was selected, having no other iron in her than what was necessarily employed in her construction, and for the ballast of a ship of her class, when in a state of ordinary. The intensity was estimated in planes* parallel to the decks,

* The term plane has been employed on the ground of convenience. Strictly speaking the decks are not planes, but curved surfaces. In the computations relative to the position of the centre of force, allowances were made for their variations of curvature.

and (excepting in a very few cases, where the peculiar form of a ship prevented) at the constant height of the binnacle above them.

To connect the different stations together, they were so arranged, as to fall in longitudinal and tranverse vertical planes, the positions of which were referred to three rectangular co-ordinate planes, having their common origin at Δ, Plate XV, fig. 1. Of these planes, that which passed in an horizontal direction through ΠΔΣ, was assumed at an elevation of 14.5 feet above the plane of the water section, the draught of water forward being 8.5 feet, and abaft 13 feet. The longitudinal co-ordinate plane passing through ΠΔΨ, Plate XVI, fig. 1, was 16 feet from the middle section, the extreme breadth of the ship being 30.5 feet; and the corresponding transverse plane passing through ΣΔΨ Plates XV, XVI, fig. 1, 2, was 41.7 feet abaft the centre of main mast, on the upper deck.

The first plane supposed to intersect the ship, was one in a vertical position, passing through its principal axis, and consequently parallel to the longitudinal co-ordinate plane. This plane intersected the poop and forecastle in $\alpha' \lambda'$, fig. 1, Plate XV.; the upper deck in $\beta'' \chi''$, fig. 2, Plate XV.; and the lower deck in $\gamma''' \lambda'''$, fig. 3, Plate XV.; the section produced thereby, being fig. 1, Plate XVI. parallel to this plane, and at eight feet from it, on the starboard and larboard sides of the ship, two other planes were supposed to pass; the former intersecting the before-mentioned decks in B′ L′, B″ L″, D‴, L‴, and the latter in $b' l'$, $b'' l''$, d''', l''', fig. 1, 2, and 3, Plate XV., and producing the sections fig. 2 and 3, Plate XVI. From the narrowness of the ship, in the after part of the poop, the fore-

part of the forecastle, and the after part of the lower deck, other planes were supposed to pass through the points A, a'; M′, m', and C‴, c''', the two former being each 7 feet from the middle section, and the latter 6.5 feet.

The before-mentioned planes having been assumed in situations, as remote as possible from the immediate influence of the iron necessarily employed in the sides of the vessel, the masts, &c.; stations denoted by the letters β', γ'', δ'', &c. B″, C″. D′, &c.; b'', c'', d'', &c., were selected in their several intersections with the decks, so as to be as free as possible from the irregularities of local attraction, and at the same time arranged in planes respectively parallel to the transverse co-ordinate plane. These parallel planes were supposed to meet the sections denoted by figures 1, 2, and 3, Plate XVI, in the lines $\gamma\,\gamma'\gamma''\gamma'''$, C C′C″C‴, $c\,c'c''c'''$ &c.; intersecting the planes of the decks, denoted by figures 1, 2, and 3, Plate XV, in the lines $c'\gamma'$ C′, $c''\gamma''$C″, $c'''\gamma'''$C‴, &c.; the longitudinal co-ordinate plane in P′ P″ P‴, X′ X″ X‴, &c.; and the horizontal plane of the same name, in $c\,\gamma$ C, $l\,\chi$ L, &c. Each point, therefore, where the intensity was to be determined, being at a given height above the deck, and in a vertical line produced by the intersection of a longitudinal and transverse plane, it's position, with respect to the three co-ordinate planes, could be accurately ascertained; and thence, if necessary, it's actual situation in space.*

The positions therefore of the different points, at which it

* It affords me pleasure to bear testimony to the liberal assistance I received from Sir BYAM MARTIN, and Sir ROBERT SEPPINGS, by furnishing me with the drawings of the various ships I found it necessary to examine, during the prosecution of these inquiries.

was desirable to ascertain the intensities, being known, the same was determined at all the stations in the three different positions of the vessel; first, when the principal section of the ship was in the plane of the magnetic meridian, and her head due north; secondly, when the same section was at right angles to that plane, and her head due east; and lastly, when the same section formed an angle of forty-five degrees with the magnetic meridian, and her head north-west.* The vessel was moored in the first situation, to discover the diversities existing in the magnetic intensity at the different stations assumed; and in the succeeding positions, to determine the changes produced in those intensities, from the immediate alteration which every individual force underwent from a change of position with respect to the primary magnetic plane.

The instrument employed for determing the intensity was similar to that denominated the apparatus of Coulomb; consisting of a magnetized cylindrical bar, two inches and a half long, and $\frac{3}{80}$ths of an inch in diameter; delicately suspended by a single fibre of the silk-worm to the extremity of an adjusting screw, which worked in the cap of the glass vessel inclosing the bar. A brass wire likewise passed through the cap, having its lower end bent into an angular form, for the purpose of placing the bar in a direction at right angles to the magnetic meridian, previous to its being allowed to oscillate.

On the different days devoted to the experiments, before visiting the ship, the time of making fifty vibrations of the

* Captain Filmore, Commander of the ships in ordinary, at Plymouth, with the utmost readiness, moored the Scylla in the above positions.

bar, was determined in the centre of a meadow, and of which the substratum was clay slate,* by a mean of six sets of experiments, performed with the utmost care; the time being registered to quarter seconds. The instrument was then taken on board, and placed in succession at the different stations previously assumed in the ship, and the mean of six sets of experiments determined at each station, with the same care as on land. The times of performing the oscillations on shore, and at each of the assumed points in the ship, necessarily gave the magnetic intensity at each station in terms of the terrestrial intensity, and which, in this case, was represented by 100.

The succeeding table contains the results of the mean intensities of the stations assumed on the poop, forecastle, the upper and lower decks of the vessel, for the positions in which she was successively moored.

* It is of importance in magnetic inquiries, to attend to the circumstances of the place in which the experiments are performed. With a delicate apparatus, like that here alluded to, the proximity of houses has a sensible effect. Even in different rooms of the same house a change of intensity has been observed, when no other iron has been near the instrument than what might have existed in the partitions and floors. When the intensity as determined in one house was denoted by 100, the same needle in another house, two hours after, gave only a result of 93.2. This was determined more than once. Each house was built on clay slate.

Table of Mean Intensities.

Direction of the Ship's Head.	Larboard Section.	Middle Section.	Starboard Section.	Mean of the three Sections.
Poop.				
North,	101.19	101.96	102.77	101.97
East,	90.76	97.88	93.14	93.93
North-west,	101.70	98.64	101.78	100.71
Forecastle.				
North,	98.44	96.87	106.30	100.54
East,	91.56	95.45	104.28	97.10
North-west,	103.86	101.89	104.31	103.35
Upper Deck.				
North,	98.51	94.45	98.51	97.15
East,	100.90	96.37	93.23	96.83
North-west,	99.55	96.46	102.72	99.58
Lower Deck.				
North,	99.77	93.56	95.83	96.39
East,	88.26	98.16	106.70	97.71
North-west,	107.34	94.03	92.40	97.92

If the preceding results be examined, it will be found that on the poop, and forecastle, the mean intensities of the three sections were the least, when the ship's head bore east; and the same property will be also found to exist in the starboard section of the upper deck, and the larboard section of the lower. But in the larboard and middle sections of the upper deck, and the middle section of the lower, they were

the least when her head was north; and in the starboard section of the lower deck when it was north-west.

Of the column devoted to the mean of the intensities of the three sections, it may be observed, that on the poop, forecastle, and upper deck, the mean results were the least when her head bore east; and on the lower deck, when it was north. The maximum result was also found on the poop, when her head was north; and on the forecastle, upper, and lower decks, when north-west. It may also be remarked, that the mean of the intensities of the three sections on the upper and lower decks were all below the assumed terrestrial intensity; and that the closest approximation to equality was found in the mean intensities of the three sections of the lower deck.

By comparing the mean intensities of the starboard and larboard sections of the upper deck, when the vessel was moored due north, it will be perceived that the results are precisely the same. This equality, however, will be found no longer to exist in the eastern and north-western positions of the ship, the larboard section having the ascendancy in the former situation of the vessel, and the starboard section in the latter. On the lower deck also, and when the ship's head was north, the difference between the mean intensities of the two lateral sections was 3.94; but when the vessel was moored easterly, this difference was increased to 18.44; and when north-westerly, it became 14.94; the excess being found on the starboard side in the former position, and on the larboard side in the latter.

Some remarkable inferences may also be drawn from a comparison of the intensities of the upper and lower decks,

in corresonding positions of the ship's head, as recorded in the following table.

Variations of Intensity from Upper to Lower Deck.				
Direction of the Ship's Head.	Larboard Section.	Middle Section.	Starboard Section.	Mean of the three Sections.
North,	+ 1.26	— 0.89	— 2.68	— 0.76
East,	— 12.64	+ 1.79	+ 13.47	+ 0.88
North-west,	+ 7.79	— 2.43	— 10.32	— 1.66

The intensity of the larboard section, for example, will be found to have increased 1.26 from the upper to the lower deck, when her head was north; to have undergone a diminution amounting to 12.64 when it became east: and afterwards to have increased 7.79 when the bow bore north-west. In the middle section likewise, a minute decrease of 0.89 was perceptible from the upper to the lower deck in her first situation; a rather greater increase of 1.79 when her head was east; and a diminution of 2.43 when it became north-west. So also in the starboard section, the magnetic influence was found to decrease from the upper to the lower deck, when her head was north, by the small quantity 2.68; and on the contrary, to undergo a rapid increase of 13.47 when it became east; and lastly, a considerable diminution, amounting to 10.32, when her head bore north-west. If the means of the intensities of the three sections be compared, with relation to the two decks, it will be found that the magnetic influence suffered a small decrement of 0.76 from the upper to the lower deck, when the vessel's head was north; but received an increment of 0.88, when her head became east;

and decreased by the quantity 1.66 when her head bore north-west. It also appears from the same table, that the alterations of intensity are the least in the middle section, and greatest in the starboard section. In the two lateral sections also it is the greatest when the ship's head was east, and in the middle section when it was north-west.

The following table also illustrates the changes of intensity which each line of stations on the respective decks underwent as the ship passed from one situation to the other. Thus, in the third column, the degree of diminutions which the mean intensity of the starboard section underwent as the head of the ship passed from north to east, amounted to 5.28; and as it's head passed from north to north-west, the increase was 4.21. It also appears from an inspection of the same table, that with the exception of the middle section on the upper deck, the changes of intensity were the greatest in the transition of the ship from north to east. The variations of intensity of the lower deck likewise much exceed those of the upper.

Change of Position of the Ship's Head.	Larboard Section.	Middle Section.	Starboard Section.	Mean of the three Sections.
Upper Deck.				
From North to East,	+ 2.39	+ 1.92	— 5.28	— 0.32
From North to North-west,	+ 1.04	+ 2.01	+ 4.21	+ 2.43
Lower Deck.				
From North to East	— 11.51	+ 4.60	+ 10.87	+ .032
From North to North-west,	+ 7.57	+ 0.47	— 3.43	+ 1.53

The succeeding table indicates the positions of the points of maximum and minimum intensity in the three sections, for the successive positions of the ship's head:

Positions of the Stations of Maximum and Minimum Intensity.

Direction of the Ship's Head.	Larboard Section.	Middle Section.	Starboard Section.
	Poop.		
North.	c' 109.09 a' 90.68	β' 102.19 γ' 101.51	C′ 108.47 A′ 96.85
East,	c' 101.73 a' 79.59	γ' 99.30 β' 96.85	B′ 94.49 C′ 91.63
North-west,	b' 104.96 c' 99.63	β' 100.84 γ' 97.69	C′ 111.25 A′ 92.02
	Forecastle.		
North,	m' 109.34 l' 87.54	γ' 100.62 x' 93.13	M′ 123.06 L′ 89.55
East,	l' 94.49 m' 88.63	x' 101.17 λ' 89.74	M′ 106.15 L′ 102.41
North-west,	m' 116.86 l' 90.87	λ' 111.77 x' 92.02	M′ 117.28 L′ 91.34
	Upper Deck.		
North,	b'' 122.46 f'' 89.92	δ'' 103.21 ι'' 86.91	L″ 103.79 F″ 93.68
East,	d'' 109.34 b'' 74.68	ι'' 99.96 θ'' 92.70	K″ 98.12 D″ 88.35
North-west,	b'' 131.07 k'' 92.02	δ'' 103.44 ι'' 88.72	L″ 108.59 F″ 90.68
	Lower Deck.		
North,	e''' 122.02 g''' 78.43	ι''' 104.73 η''' 46.60	E‴ 109.34 G‴ 78.43
East,	k''' 99.19 c''' 77.38	λ''' 109.34 η''' 81.25	C‴ 128.79 G‴ 92.12
North-west,	e''' 131.23 g''' 89.64	x''' 109.85 η''' 54.85	K‴ 102.87 L‴ 82.63

Several curious relations may be traced among the results of the preceding stations. Thus, in the middle and starboard

sections of the poop; in the three sections of the forecastle; in the starboard section of the upper deck, and in the middle and larboard sections of the lower deck, the stations at which the greatest and least intensities were respectively found, when the bow of the vessel was due north, still maintained the same property when her head was afterwards moored north-west; but in the larboard section of the poop, and the starboard section of the forecastle, the same principle was only found to hold good when the bow of the ship bore due east. In the middle and larboard sections of the upper deck, however, the points of maximum intensity were found to preserve their situations in the northern and north-western positions of the ship; but the points of minimum intensity were observed to change, approaching nearer the bow in the latter situation. A similar property with respect to the least intensities, was also found in the middle and starboard sections of the lower deck, when the vessel was moored with her head to the north and east. It may also be remarked, that in the two positions of the vessel last alluded to, the points of greatest and least intensity mutually exchanged places at some of the stations; the point of maximum intensity in the northern position of the ship becoming in the eastern that of minimum intensity, and the converse. These singular changes were observed in the middle and starboard sections of the poop; the middle and larboard sections of the forecastle, and the larboard section of the upper deck. The station η''' in the middle section of the lower deck preserved its minimum intensity in each position of the vessel.

The situation of the binnacle was nearly mid-way between

the stations γ'' and δ''; and it is remarkable, notwithstanding it is generally supposed, that this part of the ship is less influenced by iron than any other,—that the maximum intensity should have been found at the latter station, both in the first and third positions of the vessel: a proof, that sufficient care is not at all times displayed, in the employment of copper fastenings, &c., to a sufficient extent in the neighbourhood of the binnacle. A greater uniformity was also observed to prevail in the stations of the middle section, on the upper deck, when the ship's head was east, than in the other positions of the vessel.

Notwithstanding the absence of the guns, shot, and other iron stores, from the vessel under consideration, the intensities were found of a very diversified kind; and in the passage of the ship from one position to the other, the attractions were found to change in a very irregular manner;—from greater to less, and from less to greater. If therefore the different magnetic intensities, as determined in each position of the ship, be regarded as so many parallel forces, referred respectively to the three rectangular co-ordinate planes before assumed, it follows, that the position of the centre of force, corresponding to each system of intensities, may be readily determined for each co-ordinate plane, by means of the formula,

$$x = \frac{\mathrm{I}p + \mathrm{I}'p' + \mathrm{I}''p'' + \&c.}{\mathrm{I} + \mathrm{I}' + \mathrm{I}'' + \&c.},$$

in which I, I′, I″, &c. denote the intensities at the respective stations; p, p', p'', &c., the perpendicular distances of the corresponding stations, from the co-ordinate plane, to which the common centre of force is referred;* and x the unknown

* The letters α, β, &c., A, B, &c. a, b, &c. in the horizontal co-ordinate plane,

distance of the centre of force from the same co-ordinate plane. These numerical elements being respectively applied to the above formula, will determine the distance of the centre from each co-ordinate plane; and from thence, its absolute situation in each position of the vessel.

The following table contains the results of the computatations, for each co-ordinate plane, in the three positions of the vessel. In the first column is entered the direction of the ship's head; in the second the distance of the centre of force from the horizontal co-ordinate plane, corresponding to the three positions of the ship; and in the third and fourth, the distances of the same point, from the longitudinal and transverse planes. These numerical results evidently determine the position of the centre of force, for each system of intensities.

Direction of the Ship's Head.	Distance of the Centre of Force, from the Horizontal Co-ordinate Plane.	Distance of the Centre of Force, from the Longitudinal Co-ordinate Plane.	Distance of the Centre of Force, from the Transverse Co-ordinate Plane.
North.	7.58	15.98	49.23
East.	7.77	16.13	50.84
North-west.	7.64	15.89	49.84

From the preceding Table it appears, that the position of the centre of force, is not constant. This indeed, might have

figs. 1, 2, 3, Plate XVI; and N′, O′, &c., O″, P″, &c. P‴, Q‴, &c., in the longitudinal plane, figs. 1, 2, 3, Plate XV., were employed in the computations relative to the situation of the centre of force, to denote the perpendiculars here alluded to. They are retained in the diagrams, for the purpose of any farther reference.

been anticipated, from the change which the whole system of local attractions was found to undergo, in the different positions of the vessel. It may be interesting, however, to trace the course of its variation.

When the head of the ship was north, the centre of force was found at the point O, fig. 2, Plate XV., at the distance of 0.02 feet from the middle section, on the larboard side of the vessel. As the ship however moved eastward, it crossed the middle section, and was finally found at Q, on the starboard side, distant from that section 0.13 feet. When the vessel passed from north to north-west, the centre receded from O to P, on the larboard side, distant from the middle section 0.11 feet. The distance of the centre of force therefore from the middle section was a minimum when the vessel's head was north; and a maximum when east.

With reference to the transverse plane, the same point varied its position considerably, being found, when the ship's head was north, at O, fig. 2, Plate XV., or fig. 1, Plate XVI., distant 49.23 feet from it. A change of position in the vessel also, to the east or the west, caused the centre of force to advance towards the bow. When, for instance, the head bore north-west, the distance of this point from the transverse plane was 49.84 feet, as at P; and when east, 50.84 feet, as at Q;—the former position producing an increase of its distance 0.61 feet from the plane, and the latter 1.61 feet. The centre of force is therefore situated at its minimum distance from the bow, when the vessel's head is east, and attains its maximum, when due north.

Nor is the distance of the same point from the horizontal co-ordinate plane of the same constant magnitude; for when

her head was north, it was found to be 7.58 feet below it; when east, 7.77 feet, and when north-west, 7.64 feet. The depression of the centre of force below this plane is therefore the least when the ship's head is north, and the greatest when due east; the distance in the north-west position being nearly a mean between the two.

Hence it appears that the motion of the centre of force, is in that species of line which geometers denominate a *curve of double curvature.*

For the sake of practical reference it may be added, that the position of the centre of force when the ship's head is north, is nearly in the middle section, at the foremost part of the main hatch-way, and about ten inches and a half below the upper surface of the upper deck.

Of the Helicon.

The next vessel selected was the Helicon brig, mounting ten guns, and in a complete state of equipment for sea. On applying to Captain DAWKINS, her commander, he immediately caused her to be moored with her principal section in the direction of the magnetic meridian.

The first line of stations assumed was in the middle section of the ship, in which however only six points, α, β, γ, δ, ϵ, ζ, fig. 2, Plate XIX., could be obtained, on account of the midship part of the deck, from the main hatch-way, forward, being occupied by an anchor, boat, ship's gear, &c. On the starboard and larboard sides, at the distance of five feet from the middle section, two other sections were assumed, in which

fourteen stations, A, B, C, &c.; *a, b, c,* &c. were obtained, as nearly equidistant from each other, as circumstances would allow.

The station ⊙ denotes the position of the binnacle, and on each side of it were the centres Δ, Δ' of the two compasses at the distance of ten inches. From the point ⊙ as a centre, two concentric circles were described on the deck, the exterior having a radius of six feet, and the interior of three feet. Each circumference was divided into twelve equal parts at the points, 1, 1′, 2, 2′, 3, 3′, 4, 4′, &c.; and at these stations, as well as at those before referred to, in the middle, starboard, and larboard sections, the mean intensity, by six sets of experiments, was determined by the same instrument and with the same precautions as described for the Scylla.

In the middle line of stations an increase of intensity was perceptible from α to δ, at the former station it being 102.19, and at the latter 121.87; but from the last mentioned station to ϵ, the intensity declined, after which it increased to ζ, where it became 125.18. In the starboard and larboard sections, the intensities at the stations A, *a*, were found to be much greater than that at the station α in the middle section, on account of the proximity of those stations to the bulk heads formed in the quarters of the ship. At B and *b*, the decline of intensity was found to be considerable; but at the stations C, *c*, D, *d*, it was again augmented by the action of the carronades at the two after ports. At E, *e*, F, *f*, a still greater increase was observed, produced in part by the carronades at the ports opposite the last mentioned stations. The carronades were trained parallel to the side of the ship, and in no case nearer than four feet to the instrument. Those opposite the stations F, *f*, were at a greater distance; and hence it

may be inferred, that the observed augmentation of intensity was to be partly attributed to the increased attraction of the ship. At G, *g*, a farther increase in the intensity was observed, particularly at the former station, occasioned by the proximity of the flukes of the sheet anchor, which lay across the main hatch way, and bore east and west of the instrument. After passing the last mentioned stations, however, a remarkable decrease in the intensity was perceived. At H, which was but little farther distant from the flukes of the anchor than the station at F, the intensity was only 98.87; whereas at the latter station, it was 118.26. At the stations K, L, M, there was an increase of intensity, occasioned by approaching the galley; but after passing it at M, the intensity suddenly declined at N, but encreased a little at O, produced by the proximity of the station to the iron of the foremast and the anchor suspended from the starboard bow.

From the stations A, *a*, to F, *f*, the distribution of the iron, on each side of the middle section, appeared as far as a general observation could be made, to be very nearly similar; and hence the intensities at the corresponding stations of the starboard and larboard sections, approached more nearly to equality than the intensities at the stations from G, *g*, to O, *o*, on account of the chain cable, which proceeded from *g* to the larboard bow, paralled to the line of stations.

If the means of the intensities of the three sections, as far as the transverse line of stations F ζ *f*, just abaft the mainmast be taken, they will present a remarkable approximation to equality; being for the

Starboard section	-	111.92
Middle section	-	111.78
Larboard section	-	111.08;

but if the means of the intensities of the former and latter sections be determined from the stations G, *g*, before the main-mast, to the stations O, *o*, abreast of the fore-mast, this equality will be no longer preserved; the mean being for the

Starboard section 105.23,

and for the Larboard section 95.67.

Those parts of the sections therefore which are *abaft* the main-mast, have their intensities very nearly equal; but in those *before* the main-mast, the intensity of the starboard section exceeded that of the larboard in nearly the ratio of 11 to 10.

It may also be remarked, that the intensity in the after part of the ship is much more considerable than in the fore-wart part. For if the mean results of the stations from A to G, and *a* to *g* be taken and contrasted with the means of an equal number of stations from H to O and *h* to *o*, the former will be found to exceed the latter considerably. The following table contains the results.

Station from	Mean Intensity.
A to G	115.43
a to *g*	111.67
H to O	100.77
h to *o*	92.87

Hence it appears that if the vessel were divided into two parts, tranversely, between the stations G H, *g h*, the mean of the intensities of the seven stations in the after part will

be 113.55, and of an equal number in the forward part 96.82; the former exceeding the latter in the ratio of 11.7 to 10.

To avoid the influence of local attraction as much as possible, copper fastenings are commonly employed for a considerable space round the binnacle. This space however is, in most instances, much too confined in its dimensions; nor is sufficient care always displayed by the practical shipwright to avoid the mixture of iron and copper.* And to this circumstance may be attributed many of the irregularies I have noticed, when ascertaining the intensity in the neighbourhood of the binnacle, on board different ships.†

To ascertain the influence actually existing in the space surrounding the binnacle, was the object of employing the concentric circles before alluded to, and of ascertaining the intensities at the different stations assumed in their circumferences. From these it was found, that at nine of the stations in the exterior circle, the intensities were greater than those at the corresponding stations of the interior circle; but at the other three stations, viz. 2, 6, and 11, they were less.

* It may not be improper to remark, that although copper fastenings are commonly employed in fixing the planks of the deck, yet the beams which support it, being generally composed of two or three pieces, have their scharps secured by large *iron* bolts, at very short distances from each other, and several of which are in the vicinity of the binnacle. One cause at least of irregularity in the compass might be avoided, by the *general* employment of copper in the stations round the binnacle. The stanchions also in that part of the vessel should be formed of the same metal.

† HUMBOLDT, in his Voyage across the Atlantic, had much difficulty in discocovering a proper situation for suspending his dipping needle. In his personal Narratives he observes, that he finally ascertained a part of the poop to be the best place, because it appeared *nearly* free from iron, and the *small portions that existed, were very equally distributed*

This anomaly is no doubt to be attributed to the partial employment of iron in the inner circumference.

The mean of all the intensities of the stations in the lesser circumference was 102.03, and of the greater 104.40. The starboard semi-circle also of the former gave a mean intensity of 101.37, and the larboard 101.69;—approaching nearly to equality with each other. The mean intensity of the starboard semi-circle of the exterior circumference, was 105.46, and of the larboard 102.09. This excess of the intensity of the former semicircle above the latter, accords with the conclusions before deduced, relatively to the sections of the same name.

On removing the binnacle, and determining the intensity at the centre of the concentric circles, on the horizontal roof of the sky light on which the binnacle rested, the intensity was found to be 100.29, differing but little from the assumed terrestrial intensity; and hence it may be inferred, by comparing the last mentioned result, with the mean intensities of the stations in the two circumferences, that the intensity increases nearly in proportion to the radius, in a circular space surrounding the binnacle, and of which ⊙ is the centre.

At twelve inches above the surface of the compass card, the intensity at Δ was found to be 96.01, and at Δ' 93.59. At ⊙ also, immediately over the centre of the concentric circles, and at an elevation three inches greater than that last alluded to, it was 96.85. The difference between the intensities at Δ, Δ' arose from the compasses; that on the starboard side being heavy, and employed in cases when the ship is much disturbed by the sea; and that on the larboard,

being of a more delicate construction, and used in moderate weather.

It was intended to have ascertained the intensities in another circle, exterior to the greater of those before mentioned; and also to have determined the same, at a series of other points, so situated above and below the deck, as to fall in the surfaces of three concentric spheres surrounding the binnacle, and of which ☉ should be the common centre; but the wind having suddenly become fair, the ship was obliged to sail; a circumstance much to be regretted, as her gallant commander took a great interest in the inquiry, and was most anxious to afford every facility for prosecuting the subject.

Before closing these remarks relative to the Helicon, it may not be uninteresting to point out some analogies which have been traced, between the magnetic intensities observed on board her, and certain anomalies in the variation observed in Captain BUCHAN'S Voyage of Discovery.

It was remarked on board the Trent,* that when WALKER'S compass was placed on the starboard gangway, the variation was found to be 33° 52′ W.; whereas at the binnacle it was 25° 52′ W. By bringing the compass on the gangway nearer to the binnacle, the difference between those results was observed to decrease, and when it was placed on the companion, between the binnacle compasses, it exactly coincided with them.

A somewhat analogous change in the magnetic intensity, was observed on the deck of the Helicon. At the station G, on the starboard gangway, the intensity was found to be

* Journal of Science, Vol. IX.

136.50; and at ⊙, on the horizontal roof of the sky-light, and on which the binnacle rested, it was 100.29. At the station F also, the intensity was 118.26; so that a station might have been found between the points F and G, at which the intensity would have been about 131; a fourth proportional to the two variations above alluded to, and the intensity at ⊙. The observation also, that the magnitude of the difference between the two variations, gradually diminished, as the compass on the gangway was brought nearer the binnacle, affords a very striking coincidence with the intensities successively determined at the stations F, E, D, and C; they being respectively 118.26, 117.14, 110.35, and 105.20; and this analogy was farther confirmed at the station 9, in the circumference of the interior circle, nearly midway between the stations ⊙ and C, the intensity at that point being 102.87, and at ⊙, as before mentioned, 100.29.

The cause of the anomaly in the variation, was considered by the officers of the Trent to arise from "the quantity of "iron stowed round the main-mast, consisting of shot, chain "cable, spare rudder with an iron bolt, and the iron fen-"ders;" and in the present case, the increased intensity is to be attributed to the action of the sheet anchor, which was laid across the main hatchway.

A similar decrease of intensity was also traced from *g* on the larboard gangway to the centre of the binnacle, in a manner still more perfect. It may be proper to observe, however, that the direction of the Trent's head at the time the observations were made was NE $\frac{1}{2}$ N, whereas the Helicon was moored N $\frac{1}{2}$ E.

A case agreeing more exactly as to the position of the

Trent, and which exhibits in a striking manner the difference between the observations at the starboard gangway, and at the binnacle, was observed on another occasion, when her head being in the same direction as the Helicon's, the variation at the former part of the vessel was 39° 41′ W, and at the latter 21° 41′ W. The variations and intensities are not however proportional.

On another occasion, on board the Trent, the variation as determined on the starboard gangway, was found to agree with that determined at the binnacle. This observation, which would seem at first opposed to the remark before made, relative to a proportionality between the intensities at the binnacle and starboard gangway, and the variations determined at the same stations, may nevertheless be accounted for, from the sudden decline of the intensity, after passing the stations G or *g*; since it is not improbable, but between the stations G, H, *g′*, *h′*, points might have been found, at which the same intensity would have existed, as at the station ⊙.

It was also remarked, that "when Captain KATER's compass (No. 1.) was placed nearly a midships, and at a sufficient distance from the binnacle compasses to prevent attraction, the variation was found to be 31° 9′ W; and by a similar compass (No. 2.) on the ice, 25° 28′ W"."

If now the station of the compass (No. 1.) had been at δ, the intensity at that point would have had nearly the same ratio to the intensity at ⊙ (the intensity at the last mentioned point being 100.29, and the assumed terrestrial intensity 100), as the former variation has to the latter.

Of the Ariadne Frigate.

By permission of Captain Moorsom, who commanded the Ariadne, a frigate of 28 guns, the intensity was determined at a great number of stations on her different decks. At the time the experiments were performed, the vessel lay in a complete state of equipment in Hamoaze, having all her guns, shot, and other stores on board, preparatory to her departure for a long voyage. From her being under sailing orders, it was impossible to have her moored in a permanent position; and therefore from the changes which her situation necessarily underwent, in consequence of the current, one great cause of irregularity, which it is at all times desirous to avoid, in an inquiry of this kind, necessarily exercised an influence. Some useful information was however obtained from the different positions assumed by the ship, in consequence of the instantaneous and rapid change the magnetic intensity underwent at every station.

The point Δ in each of the figures contained in Plates XVII. and XVIII., was regarded as the common origin of the three co-ordinate planes. The horizontal plane passing through $\Pi\Delta\Sigma$, was assumed at the height of 16 feet 9 inches above the plane of the water section QR; the draught of water forward being 13 feet, and abaft 13 feet 9 inches, both dimensions being independent of the false keel. The longitudinal plane passing through $\Pi\Delta\Upsilon$, was supposed to be 16 feet from the middle section, and the tranverse plane $\Sigma\Delta\Upsilon$, 59 feet abaft the centre of the main-mast, on the upper deck.

The figures denoted by 1, 2, and 3, Plate XVII., represent

plans of the upper, middle, and lower decks. The lines $\alpha\, o'$, $A'\,P'$, $a'\,p'$, in fig. 1, are the intersections of the three longitudinal vertical planes with the surface of the upper deck. The first of these planes passes through the principal section of the ship; and the other two are at the equal distances of 6 feet from it, on the starboard and larboard sides of the vessel. This distance was selected, because after a careful examination of the situations of the principal attractive masses on the different decks, it seemed equally removed on the one hand from the action of the iron employed in constructing the sides of the vessel, and the guns (which on the middle deck were trained nearly parallel to the ports); and on the other, from the shot arranged round the several hatchways, the iron staunchions, and the other masses of iron existing in the middle parts of the ship.

The form of the vessel would not however admit of the extension of those planes, through the whole range of the lower deck; but at the stations F''', f''', were supposed to pass 5 feet on each side of the middle section, as far as the line of stations $H'''\ x'''\ h'''$; and from the points $E'''\ e'''$, to the line of stations $F'''\ \eta'''\ f'''$, at the distance of 2 feet and a half. The vertical plane therefore which passed through the line $\alpha'\ o'$, produced the section fig. 1, Plate XVIII. intersecting the middle deck in the line $\alpha''\ x''$; the lower deck in the line $\gamma'''\ \xi'''$; the iron ballast in $\pi\,\zeta$; the shot locker in $x'''\,\zeta\,\sigma$; and the middle course of iron tanks in $\tau\,\sigma\,\nu\,\varphi$. The starboard and larboard vertical planes which passed through $A'\,P'$, $a'\,p'$, likewise intersected the middle deck in $A''\,N''$, $a''\,n''$; the lower deck in $H'''\,N'''$, $h'''\,n'''$; the iron ballast in $\omega'\,\zeta$; and

the side course of iron tanks in $\tau' \sigma' \nu' \phi'$. These planes produced the sections, fig. 2 and 3, Plate XVIII.; the stations $E''' e'''$, F''', f''' being orthographically projected on them.

The station ξ' in the middle section was assumed midway between the binnacle compasses. In this section, no stations could be obtained on the upper deck between the points ι' and ξ', on account of the stowage of the boats, the ships gear, &c.; nor could any station be assumed on the middle deck farther forward than x'', on account of the pens for the sheep, fowls, &c., and the galley. The station θ''' was taken in the lower deck, so as to be nearly over the middle of the mass of ballast: and the station x''', just above the after bulkhead of the shot locker. The point λ''' was also assumed over the hatchway of the iron tanks; and the station λ'''', immediately below the former point, and about two feet above the surface. The station ν''' was taken as nearly as possible to the foremost extremity of the tanks.

The diagram denoted by fig. 1, Plate XIX., is a horizontal section of the ship, passing through the upper surface of the middle tier of tanks. The tanks are denoted by the letters $\sigma\, t\, u\, \nu\, u'\, t'$, and the ballast by $s\, r\, q\, q'\, r'\, s'$; the iron pigs composing the mass being represented by the small rectangular parallelograms. The single dot in the middle of some of the rectangular spaces, implies, that the ballast in that part is one pig deep; two dots, two pigs deep; three dots, three deep, and so on. The rectangular space $v\, w\, v'\, w'$, is a horizontal section of the well which contains the main mast, chain pumps, &c.; and the parallelogram $w\, x\, w'\, x'$, that of the shot locker. The dotted lines passing through $E''' e'''$, $F''' f'''$, and $N''' n'''$, may be regarded as continuations of the

longitudinal sections alluded to, in fig. 3, Plate XVII., and are introduced into the diagram, to exhibit the relative situations of the stations on the lower deck, with respect to the ballast and tanks.

On the after part of the upper deck, as shown in fig. 1, Plate XVII., the stations in the starboard and larboard sections, were less numerous than in the middle section, on account of the difficulty of finding points, as far removed as possible from the influence of the carronades, and which in this part of the ship were at right angles to its sides. In the after part of the middle deck, or in the Captain's cabins, the stations were more numerous; fifteen having been selected for the purpose of particularly discovering what varieties of intensity existed in this part of the ship, in order to form an estimate how far the magnetic changes were likely to affect a chronometer when placed in the cabin. With the same view the stations in the after part of the lower deck, fig. 3, Plate XVII., were assumed; because in this part of the vessel are the births of the lieutenants and master, and in which chronometers are sometimes deposited.

In the middle section, on the upper deck, it was found that the intensity attained its maximum at η', at the distance of 4 feet 5 inches before ε', the position of the binnacle. After passing the point of greatest intensity, a sudden declension of its power was observed at ι', and which was farther continued to ξ', where it became a minimum, after which it received an augmentation at o'. At ε', the station between the binnacle compasses, a great uniformity was observed in the results of the oscillations; and it was farther remarked, that the intensity at the station ε', was an exact mean between

the intensities at the stations ϵ', and η'. The intensities at and abaft the point η', possessed intensities greater than the assumed terrestrial intensity; and from that station to the forecastle, the magnetic influence was found to be less.

In the same section on the main deck, the intensity increased from 97.90 to 108.47, in the short interval from α'' to β'', in consequence of the iron tiller, which was below the deck, and immediately in the plane of the section. At the station γ'' the intensity underwent a small declension, and at δ'' a still farther decrease, the south pole of the oscillating bar dipping towards the extremity of the tiller, the station being over it. At ϵ'' the intensity again increased, after which it declined progressively to x'', the last point in the section it was possible to examine.

On the lower deck, and in the same vertical plane, it was remarked that the intensity at γ''' was very nearly the same as at the corresponding station on the main deck. At the points ϵ''' and η''' the attractive influence increased, but diminished at the stations θ''' and x'''. At this point also the north pole of the oscillating cylinder was drawn five degrees below its horizontal direction; an effect produced by the proximity of the station to the after part of the shot-locker. At the station χ''', over the central tank, the intensity increased to 118.97; but at χ'''', immediately below the last mentioned station, and only twenty inches above the surface alluded to, the intensity was less by the quantity 16.33. At μ''' the attraction again declined; but at γ''', nearly over the foremost extremity of the tanks, attained to 128.15, its maximum.

In the starboard section, on the upper deck, the least intensity was found at F', in the same transverse vertical plane

as the maximum intensity of the middle section on the same deck. The greatest intensity also was found at L′, situated, as will hereafter appear, in the same transverse vertical plane as the maximum intensity of the larboard section of the main deck, and the greatest intensities of the middle and starboard sections of the lower deck. The former effect was produced by a carronade, which was stowed away amidships; and in the latter case, from the oscillating bar being between two carronades. The transverse plane also, in which the point F′ of minimum intensity was found, was observed to be at nearly the same distance from the extreme point of the stem, as a similar plane passing through the point L′ of maximum intensity was from the extreme part of the stern.

In the corresponding section on the main deck the greatest intensity was found at B″, in the same transverse vertical plane as the maximum intensity of the middle section, and the minimum intensity of the larboard section on the same deck. The minimum intensity was also discovered at I″, in the same transverse plane as the minimum intensity of the larboard section on the lower deck. The intensities of the stations in this section very much exceed those of any other line of stations, their mean being 115.79. At the points A″, B″, C″, D″, E″, the attractive power was very great, the mean of the intensities at those stations being 134.92; whereas the mean of the five succeeding stations was only 95.40, and that of the remaining points L″, M″, N″, 117.88. The magnetic changes in this section were very considerable; and it was farther remarked, that in the five positions first alluded to, no guns were near the line of stations; and that therefore the great degree of intensity found in this part of

the vessel arose from the iron tiller, and the knees and braces employed in securing the stern. The increased intensity of the three last mentioned stations, above the five which preceded them, may be attributed to the chain cable, which commenced at L″, and to the galley, which was opposite to M″.

In the same section also, on the lower deck, the maximum intensity was observed at L‴, in the same transverse section as the maximum intensity of the middle section on the same deck, the corresponding intensity of the larboard section on the main deck, and a similar intensity of the starboard section on the upper deck. The least intensity likewise was found at H‴, in the prolongation of the after part of the shot-locker, and in the same vertical transverse plane as the minimum intensity of the middle section, and the greatest intensity of the larboard section on the same deck.

At the stations *m*′, *n*′, in the larboard section on the upper deck, a close approximation to equality was perceptible in the observed intensities; and at the corresponding stations M′, N′, in the other lateral section, a perfect equality was found. At the remaining stations in those sections, however, no analogy could be traced among the results. The maximum intensity was found at *d*′, in the same transverse plane as the least intensity of the middle section on the main deck; and the minimum intensity at O′, in the same plane as the least intensity of the middle section on the upper deck.

In the same section, on the main deck, the intensities were no less remarkable, when contrasted with the results of the starboard section on the same deck. The greatest intensity was found at *l*″, in the same vertical plane as the maximum

intensity of the starboard section on the upper and lower decks, and the corresponding intensity of the middle section on the latter deck. The minimum intensity was at b'', in the same plane as the greatest intensities of the middle and starboard sections on the main deck.

On the lower deck, and in the same longitudinal plane, the most striking difference in the intensities of the starboard and larboard sections was found at the stations H''', h'', the former being 69.96, and the latter 155.63; the difference between the bearings of the ship's head at the two observations being only a point and a quarter. The last mentioned intensity was also found to be the greatest in the whole section; and was situated in the same transverse plane as the minimum intensities of the middle and starboard sections on the same deck. The line of stations H''' x'' h'' was immediately over the after part of the shot-locker. The least intensity was discovered at e'', in the same vertical plane as the minimum intensity of the starboard section on the main deck.

The following table exhibits the maximum, minimum, and mean intensities of the stations assumed in the three longitudinal planes, arranged according to the sections in which they are found.

Intensities according to the Sections.

Name of the Deck.	Station.	Maximum Intensity.	Direction of the Ship's Head.	Station.	Minimum Intensity.	Direction of the Ship's Head.	Mean Intensity.
Starboard Section.							
Upper Deck,	L′	107.98	NNE	F′	89.64	SSE	100.69
Main Deck,	B″	154.96	SE by E½E	I′	92.50	W½S	115.79
Lower Deck,	L‴	115.49	SW½W	H‴	69.96	W by S½S	96.58
Middle Section.							
Upper Deck,	η'	119.54	NE by N½E	ξ'	80.29	N by E¾E	100.98
Main Deck,	β''	108.47	SE by E½E	α''	97.90	E by N½N	103.78
Lower Deck,	γ'''	128.15	SW¾S	x'''	74.83	W	104.04
Larboard Section.							
Upper Deck,	a'	118.68	SSW¼W	o'	82.80	N by E½E	96.58
Main Deck,	l''	121.28	SW by S	b''	71.07	E½N	98.70
Lower Deck,	h'''	135.63	W¼S	i''	95.71	W½N	114.27

It also appears from the preceding table, that the mean intensity of the stations assumed in the middle section increases from the upper to the lower deck; the intensity augmenting however in a greater ratio from the upper to the main deck, than from the main to the lower deck. The mean intensity likewise in the starboard section increases considerably from the upper to the main deck; but from the last mentioned deck to the lower it diminishes in a still greater ratio. In the larboard section, however, there is a small increase of the intensity from the upper to the lower deck, amounting to nearly the same quantity as in the middle section; but from the main to the lower deck, the rapidity of increase is very considerable.

The intensities also in the line of stations assumed in the middle section on the main deck, approached much nearer a state of uniformity than those of any other; the difference between the greatest and least intensities being only 10.57; whereas the difference on the same deck, in the starboard section, amounted to 61.86, and in the larboard section 50.21. In these sections also the greatest uniformity was perceptible on the upper deck.

It is worthy of remark, moreover, that the mean intensities of the stations in the middle section on the different decks, accord very nearly with the means of the greatest and least intensities in the same section on the same decks;—a principle not to traced in the other sections.

By considering the different results according to the order of the sections on the same deck it will be perceived, that the mean intensity on the upper deck increases in a small degree from the starboard to the middle section, from which it undergoes a more considerable decline to the plane on the larboard side; but on the main deck, the average intensity decreases rapidly from the starboard to the middle section, and from thence, by a less rapid diminution, to the larboard plane. On the lower deck, however, the case is the reverse; the mean intensity being the least in the starboard section, from which it increases to the middle section, and finally attains its maximum in the larboard section.

The diversities however which exist in the intensities at the corresponding stations of the three sections abaft the mizen mast, merits a particular examination, on account of the effects which such varied and uncertain influences must

have on the rates of chronometers. To afford some idea of the diversified nature of these intensities, the following results have been selected from many others, and which sufficiently prove that the action of induced magnetism in this part of the vessel is subject to the most singular and remarkable variations.

Experiments in the Captain's Cabin.								
Larboard Section.			Middle Section.			Starboard Section.		
Station.	Intensities.	Direction of the Ship's Head.	Stations.	Intensities.	Direction of the Ship's Head.	Stations.	Intensities.	Direction of the Ship's Head.
a''	92.31	ENE	α''	97.90	E by $\frac{1}{2}$ N	A″	137.38	E by N $\frac{1}{2}$ N
b''	71.07	E $\frac{1}{2}$ N	β''	108.47	SE by E $\frac{1}{2}$ E	B″	154.36	SE by E $\frac{1}{2}$ E
c''	79.35	NE by E	γ''	107.36	NE by E	C″	128.15	ENE
d''	89.83	NE by E	δ''	98.87	NE by E	D″	127.84	NE $\frac{1}{2}$ E
e''	80.45	E by N	ι''	108.22	E by N $\frac{1}{4}$ N	E″	126.89	E $\frac{1}{2}$ S
Mean	82.60		Mean	104.16		Mean	134.92	

At the time the above experiments were performed, the iron tiller was exactly amidships; and the iron knees and braces round the stern and sides of the ship appeared to be so equally distributed, that it seems difficult to account for the different intensities recorded in the table, on any other principle than as arising from the constant fluctuations of the ship's head. That this at least is one of the primary causes of the observed anomalies, will be rendered exceedingly probable, from some observations which will be speedily advanced. Of the starboard section it may be remarked, that all the results very much exceed the assumed terrestrial intensity; but in the middle section, at two of the

stations only, were the measures of the magnetic influence found below that of the earth; and in the larboard stations, the whole of the results were found much below. If the mean of the five intensities in the latter section be denoted by 10, that of the corresponding stations in the middle section will be 12.6, and of the starboard 16.3*

The magnetism arising from position is, it is well known, of a very variable kind; developing its intensity in some situations of an iron mass, with singular energy and force, and in others exhibiting only an action of the feeblest kind. Those changes also, manifesting their influence in an instan-

* The following observations made by Captain MOORSOM, on board the Ariadne, in Simon's Bay, at the Cape of Good Hope, will exhibit the anomalous results of a dipping needle when employed on ship board.

Experiments performed in the After Cabin.			
	Larboard Side.	Amidships.	Starboard Side.
Station,	b''	β''	B''
Dip,	51° 55′	52° 10′	51° 5′

In the fore cabin, at the station γ'' amidships, the dip was found to be 47° 47′.

On the quarter-deck, at the station ξ', between the binnacle compass, and on the stand for the azimuth compass, it was found to be 48° 15′.

In the after cabin at the station β'' amidships, the dipping needle showed 59° 25′, when placed in a plane inclined 45° to the magnetic meridian; but in the fore cabin at γ'', the instrument with the same azimuth gave only a result of 56° 15′.

In the stern sheets of the barge, on the booms, the dip was found to be 50° 22½′.

The dip determined in a house in Simon's Town, distant thtee quarters of a mile from the ship, was 48° 23′½, agreeing within 8′½ of that determined between the binnacles.

The latitude of the ship was 34° 11′ 38″ S., and longitude 18° 28′ 35″ E. Variation 28° W.

taneous and rapid manner, may be supposed to have unfolded some anomalies in the course of the present experiments, from the numerous alterations which took place in the bearings of the ship's head, and the consequent change of intensity of every mass of iron.

In a preceding page, a table was introduced for the purpose of exhibiting the remarkable anomalies existing at different stations of the longitudinal sections abaft the mizen mast; but the succeeding observations relate to the changes which the intensity of the *same* station underwent in consequence of alterations in the bearings of the ship's head. At the starboard station B″, for example, the intensity was found to be 154.36, when the direction of the ship's head was SE by E½E, but declined to 52.85, when it bore W½N. At the larboard station *b*″, likewise, the intensity was found to be 71.07, when the head was directed E½N, and increased to 147.64, when it turned to W½N. At the latter station also, when the vessel's head was NW by N¼W, it amounted to 112.81. The intensity at the station *β*″ in the middle section was likewise found to vary, but not in the same degree as the stations in the starboard and larboard sections. When the head, for instance, bore SE by E½E, the intensity at the last mentioned point amounted to 108.47; and when, by the influence of the tide, the same part of the vessel was directed W by N, it declined to 103.79; and when by the continued change of the ship's position the head became N½W, a farther declension took place to 99.43. These results however may be more clearly understood by a reference to the following table.

Station.	Intensity.	Direction of the Ship's Head.
	Starboard Section.	
B''	154.36 52.85	SE by E $\frac{1}{2}$ E W $\frac{1}{2}$ N
	Middle Section.	
β''	108.47 103.79 99.43	SE by E $\frac{1}{2}$ E W by N N $\frac{1}{2}$ W
	Larboard Section.	
b''	71.07 147.64 112.81	E $\frac{1}{2}$ N W $\frac{1}{2}$ N NW by N $\frac{1}{4}$ W

In the succeeding table will be found the different stations, at which the most considerable variations were observed in the magnetic intensity, during the uncertain positions of the vessel; and also the representative numbers for the intensities, corresponding to the respective bearings of her head. The fourth column exhibits the observed variation of intensity at each station; and the last, the simultaneous change of the ship's head in point of the compass.

Stations.	Intensity.	Direction of the Ship's Head.	Changes of Intensity.	Change of Ship's Head in Points.
		Starboard Section.		
C″	129.92 126.42	NE by E E by N	— 3.50	2
H″	95.71 94.79 92.70	WSW ½ W W by S W by S ½ W	— 0.92 — 2.09	½ ½
L″	119.68 116.31 113.07 110.48	WSW ½ W W by S W by S ½ W W by S ¾ W	— 3.37 — 3.24 — 2.59	½ ¼ ¼
E‴	101.73 100.50 99.30 94.49	SSW ½ W SW by S SW ½ W SW by W ¼ W	— 1.23 — 1.20 — 4.81	½ 1¼ ¼
		Middle Section.		
ε''	104.96 111.39	E ½ S ENE	+ 6.43	2½
η'''	115.62 113.74 113.07 111.77 110.10	S S by W ½ W SSW SSW ¼ W SSW ½ W	— 1.88 — 0.67 — 1.30 — 1.67	1½ ¼ ¼ ¼
ν'''	125.34 131.07	SW SW by S ½ S	+ 5.73	1½
		Larboard Section.		
a''	90.68 96.33 89.74	ENE ENE ½ N E by N ½ N	— 5.65 — 6.59	½ 1
b''	65.68 70.29 77.53	E by S E ½ N ENE ½ E	+ 4.61 + 7.24	1½ 2
k''	118.12 120.99	SW by W ½ W SW	+ 2.87	1½

Some analogy may be traced between the magnetic changes recorded in the above table, and those which an iron mass undergoes when its situation is altered with respect to the magnetic meridian. If, for example, a number of points be assumed in an iron mass in the direction of the magnetic

meridian, it is well known that the intensity of each point will diminish, as the stations are brought into a position at right angles with the first; and on the contrary, an increase of intensity will be observed when they are removed from the latter situation to the former. Changes of a similar kind may be distinctly traced in all the preceding observations. At either stations, ϵ'' and b'', for example, an increase of intensity was perceived as the ship moved from the position when her head was easterly, to that when it bore due north; and a similar change was observed at γ''' and k'', as her bow passed from a westerly direction to one exactly south. So also at the station H''', an augmentation of intensity was remarked as her head moved from a westerly to a southerly position; and on the contrary, a diminution of intensity as the direction of her bow was changed from south to west.

In like manner at the station d', an increase of the magnetic power was discovered as the vessel passed from east to north; but a diminution of intensity as she moved from north to east. At the stations η''', H″, L″, E‴, and K‴, a decrease of intensity was likewise apparent as her head was carried by the current from south to west; and a change of a similar kind as it varied from north to east.

From these considerations it appears, that the changes in the magnetic intensity, at any station in the vessel, are regulated by laws analogous to those which influence simple masses of iron.

The variations of intensity however at the several stations were of a very unequal kind. In some parts of the ship, the alteration of a quarter of a point in the direction of her head was productive of a greater change than the variation of an entire point at some other stations. Nor does the change of

intensity at the *same* point, appear to be proportional to the alteration in the direction of the ship's head, Such inequalities must however be considered as necessary consequences of the irregular distribution of the iron, and of its inequality of action.

The position of the common centre of force of all the determined intensities may be referred to the three rectangular co-ordinate planes, by the same method as that employed for the Scylla; and the application of the different numerical elements to the proper formula, furnishes the following results:

Value of S″ V″, being the Distance of the Centre of Force from the Horizontal co-ordinate plane.	Value of S″ W″, being the Distance of the Centre of Force from the Longitudinal co-ordinate plane.	Value of S″ T″, being the Distance of the Centre of Force from the Transverse co-ordinate plane.
9.45	16.08	59.81

By comparing these results with the actual dimensions of the ship, it appears that the common centre of force of all the intensities is at the point S″, 59.81 feet from the transverse co-ordinate plane near the fore part of the main-mast, and 4.71 feet abaft the prolongation of the after part of the shot-locker. From the horizontal co-ordinate plane the same point appears to be 9.45 feet distant, and hence 1.25 feet above the plane of the main deck. Its distance also from the longitudinal co-ordinate plane is .08 feet on the starboard side; and practically considered, may be regarded as in the middle section of the ship.

Of the Impregnable of 104 *guns.*

Many interesting results were obtained on the different decks of this fine ship. Three longitudinal planes, similar to

those adopted in the Ariadne and Scylla, were supposed to intersect her different decks from the poop and forecastle, down to the hold and kelson; and stations were assumed in each, at which the intensities were determined. Figure 3, Plate XIX., denotes the section formed by the plane passing through the principal axis; and the letters A, B, C, D, E, F, G, H, I, the stations assumed in it on the poop, quarter deck, and forecastle. The dotted lines proceeding from each of these stations, intersecting the successive decks, &c. of the vessel, represent the positions of the points at which the intensities of the other parts of the section were determined. In the following table, the numerical results of the intensies are recorded; those of the other sections having been suppressed, on account of the length to which the enquiry has extended.

	STATIONS.									
Names of the Decks.	A.	B.	C.	D.	E.	F.	G.	H.	I.	Mean of the Intensites.
Poop, - -	100.00	100.66	104.19	—	—	—	—	—	—	101.62
Quarter Deck and Forecastle,	98.69	91.29	104.54	98.91	94.94	98.27	100.44	109.14	96.58	99.20
Main Deck,	96.58	89.96	102.91	100.33	98.70	93.93	92.64	118.03	100.00	99.23
Middle Deck,	88.76	94.83	80.73	100.88	102.69	100.22	94.43	94.63	96.06	94.80
Lower Deck,	—	102.34	108.88	108.27	95.04	101.22	88.67	90.72	100.00	99.39
Orlop Deck,	—	—	—	91.67	95.75	75.08	93.23	101.89	97.42	92.51
Hold, - -	—	—	—	96.37	83.00	78.47	87.67	121.77	87.22	92.42
On the Kelson in the Pump Well,	—	—	—	—	—	107.05	—	—	—	107.05
Mean of the Intensities,	96.01	95.82	100.25	99.40	95.02	93.46	92.85	106.03	96.21	—

From an inspection of the preceding results it appears, that in passing from the mean intensity of the poop, to that of the quarter deck and forecastle, there is a decrease amounting to 2.42; from the deck last mentioned to the main deck, a feeble increase of 0.03; from the main to the middle deck, a diminution of 4.43; from the middle to the lower deck, an increase of 4.59; from that to the orlop deck, a decrement of 6.88; from the orlop deck to the hold, another decrement of 0.09; and from the mean intensity of the hold to the single intensity determined on the kelson, an increment of 14.63. Hence it appears, that the greatest mean intensities are found at the extremes of the series; that the mean results of the quarter deck and forecastle, and main and lower decks, are very nearly the same; as are also those of the orlop deck and hold. The mean intensity of the middle deck is also very nearly a mean, between the mean of the intensities of the three first mentioned decks, and of the two latter. The mean of the deck from the poop to the middle deck inclusive, is 98.71; and of the lower and orlop decks, hold and kelson 97.84. The mean of all the decks is 98.28, being 1.72 less than the assumed terrestrial intensity.

In taking the means of the lines of stations vertically, it will be perceived, that the maximum intensity is attained at the line of stations denoted by H, and the minimum at the adjacent stations G. If also the mean of the middle column of intensities E be adopted, the result will be very nearly the same as the mean of the intensities of the middle deck; and likewise, that the mean of the results of the columns A, B, C, D, abaft the column E, is very nearly equal to that of the columns F, G, H, I, on its other side; the former

being 97.89, and the latter 97.13. It is proper to add, that this vessel had not her lower deck guns on board.*

As examples of the diversities of intensity existing in different ships at stations similarly chosen, and determined by the same instrument, the following are selected. The primary position assumed in each vessel was that of the binnacle, as A, Plate XIX., fig. 4. The right line MN, which passes through this point, is supposed to denote the principal axis of the ship, M being the after part. The lines CD, EB, which likewise pass through A, and form angles of 45° with the axis, are each 16 feet in length. At the extremities of those lines, and also at the station A, the intensities were determined by a mean of six experiments, and the results are recorded in the following table. The oscillating bar was elevated in each case at the constant height of 20 inches above the deck.

	Caledonia, 120 guns.		St. Vincent, 120 guns.		Malta, 84 guns.		Cornwallis, 74 guns.		Nereus, 46 guns.		Melampus, 46 guns.	
Stations.	Intensity.	Direction of the Ship's Head.	Intensity.	Direction of the Ship's Head.	Intensity.	Direction of the Ship's Head.	Intensity.	Direction of the Ship's Head.	Intensity.	Direction of the Ship's Head.	Intensity.	Direction of the Ship's Head.
A	101.74	N by E	104.49	SW by S	94.39	SE	89.74	SSW	96.95	N by W	100.95	S by W
B	108.97	N by E	102.64	S by W½W	102.07	SE by S½E	80.37	SSW	107.98	N by W½W	105.92	S by W½W
C	103.68	N by E	99.08	SSW	90.02	SE by S	97.48	SSW	100.84	NNW	96.64	S½W
D	105.43	N by E	100.62	S by W	106.51	SE by S	99.85	SSW	103.44	NW by N	104.26	S by W
E	103.10	N by E	98.01	S by W	97.29	SE by S½E	96.42	SSW	95.40	NW	104.96	S
Mean	104.58	Mean	100.97	Mean.	98.06	Mean.	92.71	Mean.	100.92	Mean.	102.55	

* The assistance I received from the officers of the Impregnable, in enabling me to obtain suitable stations for determining the intensity, demand from me the warmest acknowledgments.

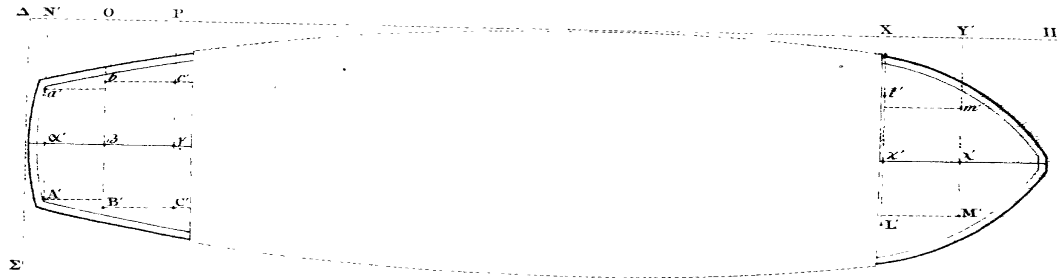


Fig. 1.

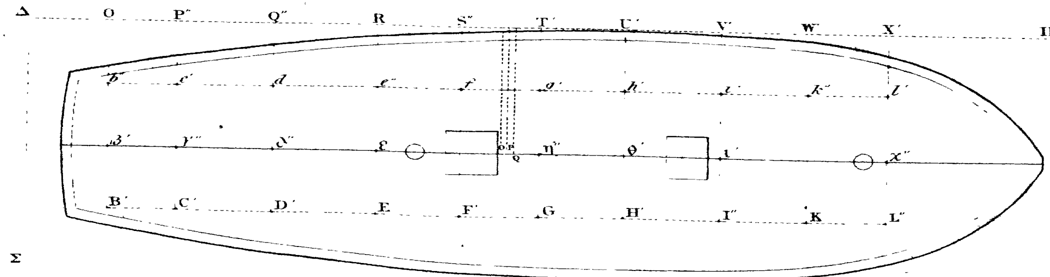


Fig. 2.

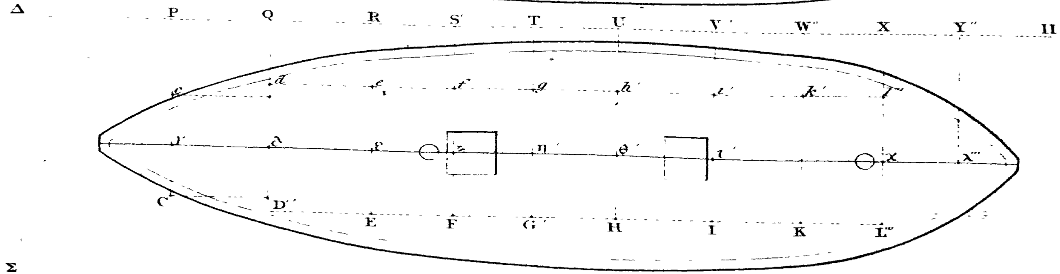


Fig 3.

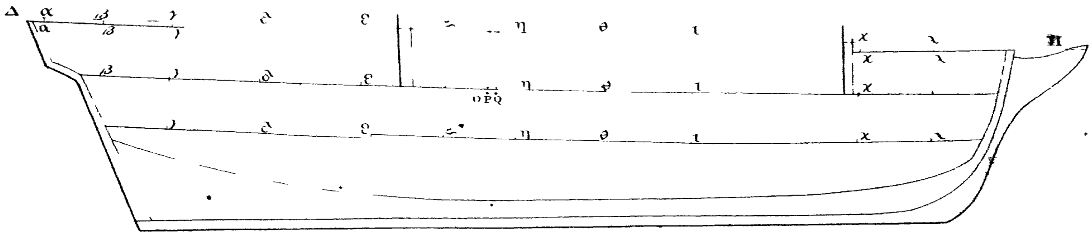


Fig 1

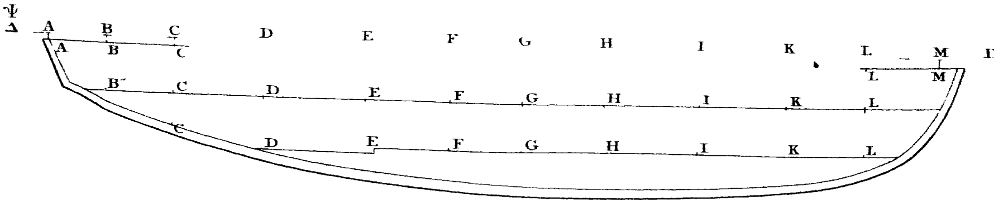


Fig 2

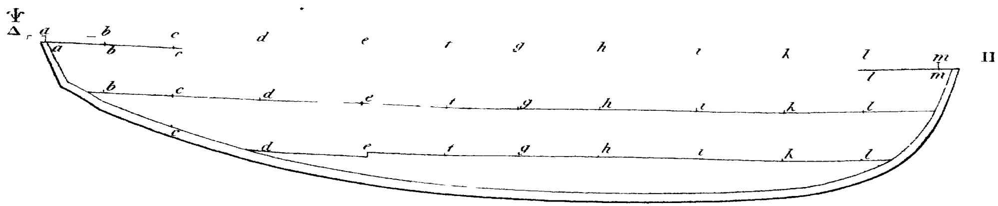


Fig 3

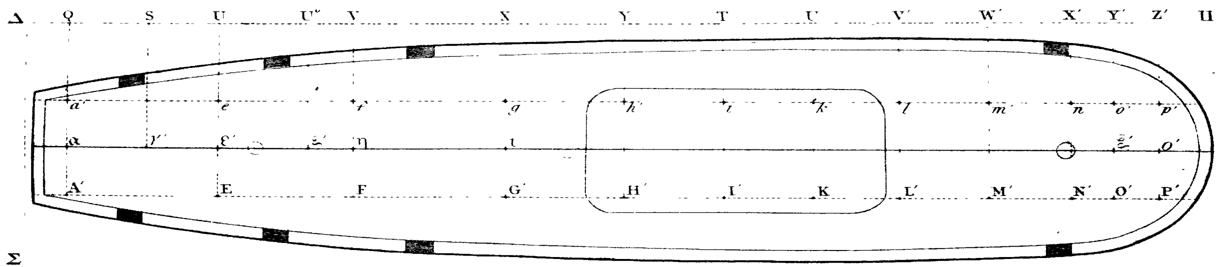


Fig. 1.

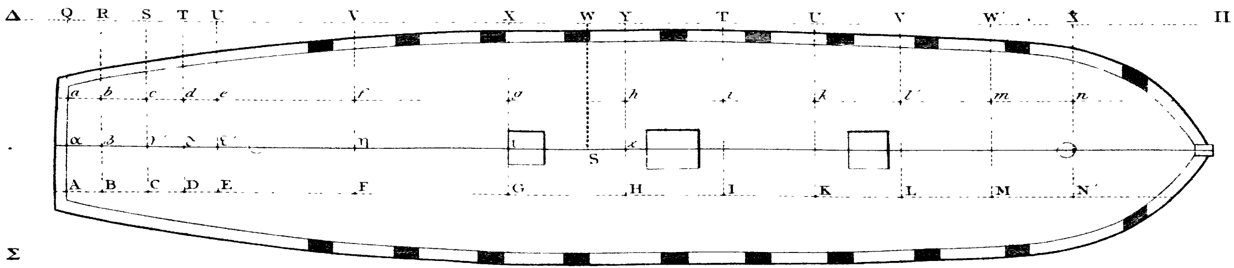


Fig. 2.

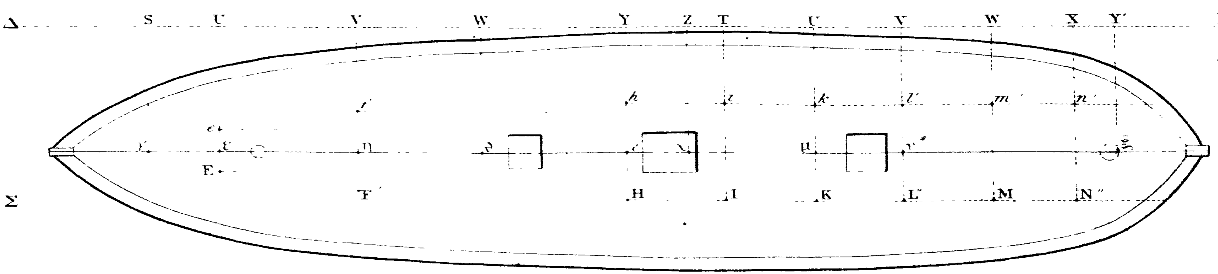


Fig 3.

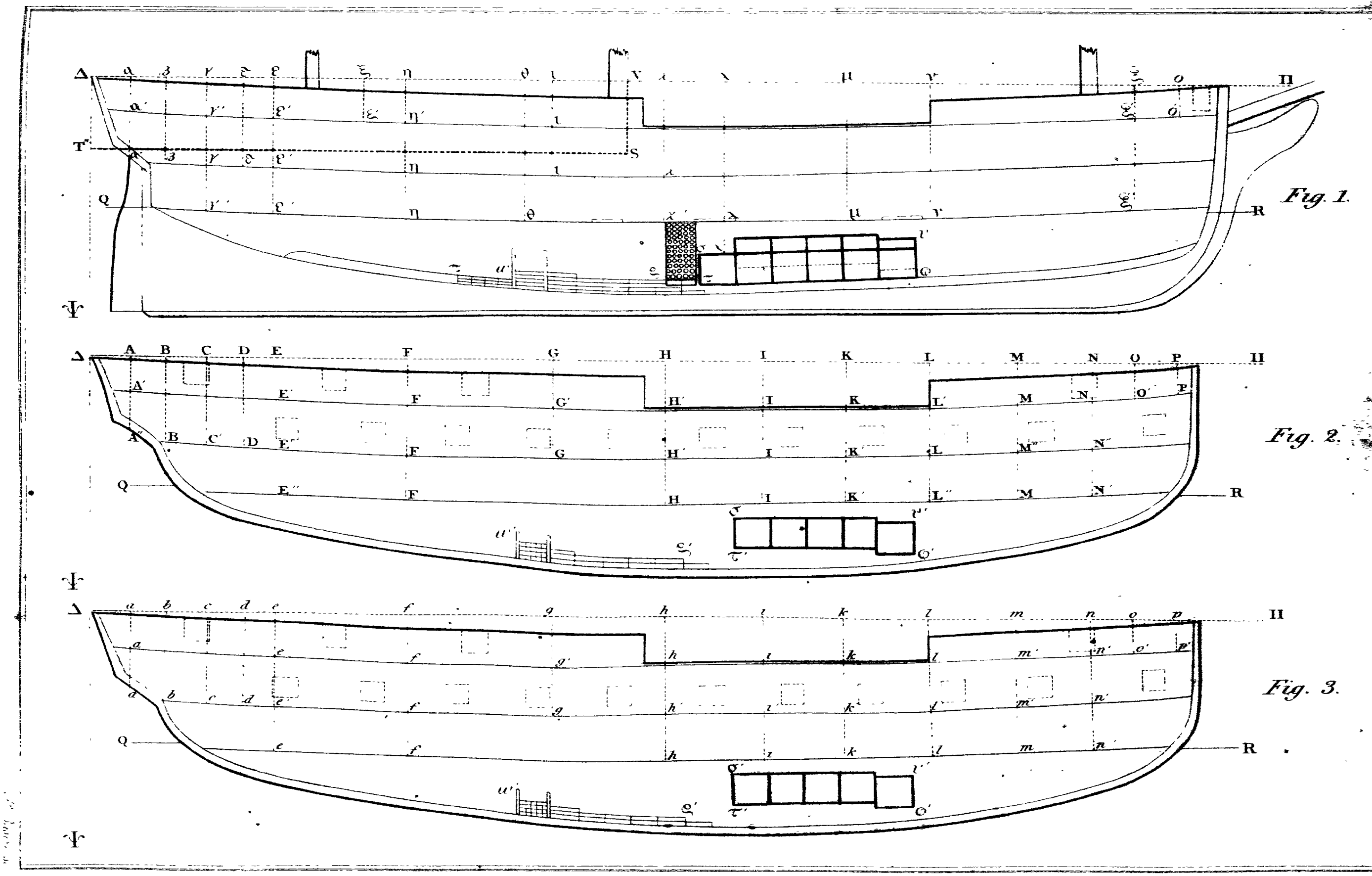


Fig. 1.
Fig. 2.
Fig. 3.

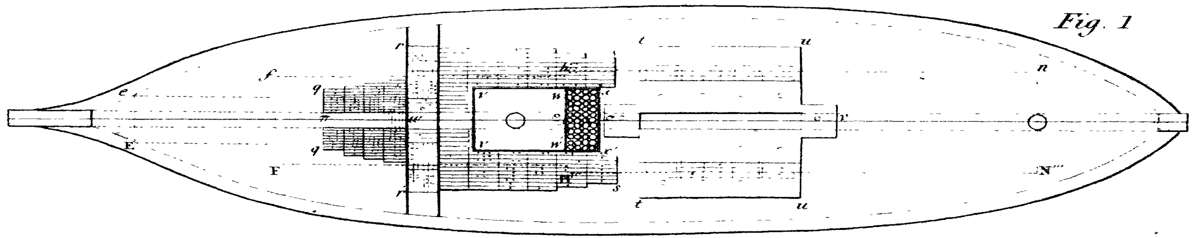


Fig. 1

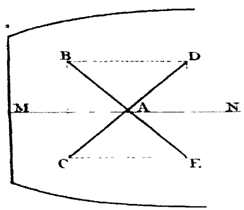


Fig 4

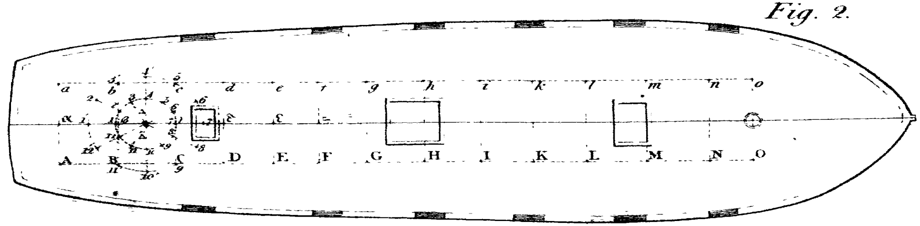


Fig. 2.

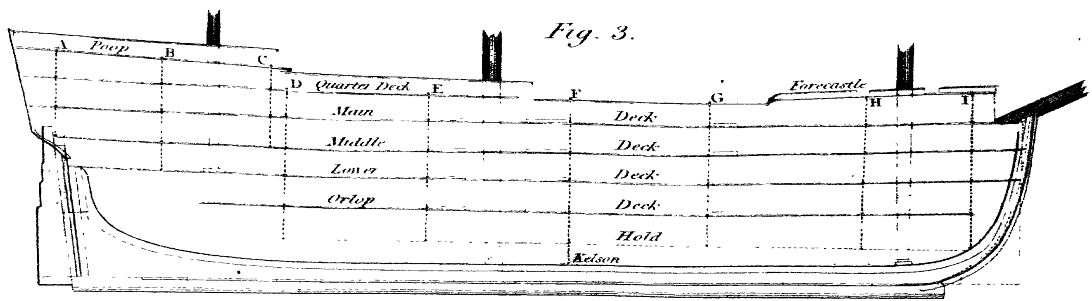


Fig. 3.

The diversities which prevail in the above intensities, must be attributed to the unequal distribution of the iron in the different ships, and to the varied directions of their heads. No definite relation appears to exist between the magnitudes of the ships, and the several results. The example of he Cornwallis deserves notice, as having the intensities of all its stations singularly below the assumed terrestrial intensity. It may be proper to add, that these ships were in a state of ordinary, in Hamoaze, and that the intensities are therefore due to the hull alone.

As a useful practical remark it may be added, since some difference of opinion has existed on the subject, that during the course of these experiments it has appeared, that the changes and diversities of intensity on board small ships of war, are more considerable than those which take place in vessels of a larger class.

The mean latitude of the positions of the ships may be regarded as $50° 22' 53''$ N ; and longitude $4° 14' 44''$ W.

Plymouth, May 1, 1823.

XVIII. *Experiments on the elasticity and strength of hard and soft steel. In a Letter to* THOMAS YOUNG, *M. D. For. Sec. R. S. By Mr.* THOMAS TREDGOLD, *Civil Engineer.*

Read March 25, 1824.

SIR, *London, Dec. 16, 1823*

IF a piece of very hard steel be softened, it is natural to suppose that the operation will produce a corresponding change in the elastic power, and that the same load would produce a greater flexure in the soft state than in the hard one, when all other circumstances were the same. Mr. COULOMB inferred from some comparative experiments on small specimens, that the state of temper does not alter the elastic force of steel; and your Experiments on Vibration led to the same conclusion (Nat. Philos. II. 403). But the subject appeared to require further investigation, and particularly because it afforded an opportunity of ascertaining some other facts respecting steel, which had not been before examined.

In making the experiments which I am about to describe, each bar was supported at its ends by two blocks of cast iron. These blocks rested upon a strong wooden frame. The scale to contain the weights was suspended from the middle of the length of the bar, by a cylindrical steel pin of about $\frac{3}{8}$ths of an inch in diameter. And as in experiments of this kind it is desirable to have the means of raising the weight from the bar, without altering its position, in order to know when the load is sufficient to produce a permanent change of structure, I have a powerful screw with a fine

thread fixed over the center of the apparatus, by which the scale can be raised or lowered, when the cords on which the screw acts are looped on to the cross pin by which the scale is suspended.

To measure the flexure, a quadrantal piece of mahogany is fixed to the wooden frame; two guides are fixed on one edge of the mahogany, in which a vertical bar slides, and gives motion to an index. The bar and index are so balanced, that one end of the bar bears with a constant pressure on the specimen, and the graduated arc over which the index moves is divided into inches, tenths, and hundredths; and thousands are measured by a vernier scale on the end of the index. There is a screw at the lower end of the vertical bar, by which the index is set to zero, when necessary. Plate XX.

The first trials were made with a bar of blistered steel of a very good quality. It was drawn out by the hammer to the width and thickness I had fixed upon, and then filed true and regular. It was then hardened, and tempered to the same degree of hardness as common files.

The total length of the bar was 14 inches; the distance between the supports 13 inches; the breadth of the bar 0.95 inches, and the depth 0.375 inches; the thermometer varied from 55° to 57° at the times of trial.

	lbs.		inches.
With a load of	54	the depression in the middle was	0.02
	82	- - - - - -	0.03
	110	- - - - - -	0.04

The last load remained on the bar some hours, but produced no permanent alteration of form.

The temper of the bar was then lowered to a rather deep straw yellow, and it was tried again; when the same loads produced exactly the same flexures as before.

The temper was then lowered till the colour was an uniform blue, or spring temper; and the trials were repeated with the same loads; but the flexures were still the same.

It was now heated to redness and very slowly cooled. In this state the same loads still produced the same flexures; and the load of 110 lbs. caused no permanent change of form.

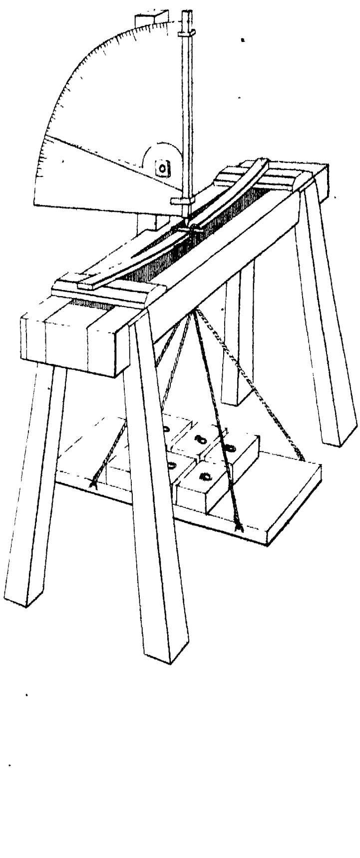
The bar was hardened again, and made very hard; in this state the same loads produced the same flexures; and

	lbs.		inches.
with a load of	300	the depression in the middle was	0.115
	350	- - -	0.130
	380	- - -	broke.

When the bar was relieved from the load of 350 lbs. it retained a permanent flexure of 0.005 inches, which increased to 0.01 with the addition of 10 lbs. to the load.

I found that a bar of much greater length might be tempered without difficulty, and therefore had another bar made of the same kind of steel; the length of which being 25 inches, about double the flexure could be given with the same strain upon the material, and therefore any small degree of difference in the elastic force might be more easily detected, for the preceding experiments are sufficient to show that if there be any difference, it must be extremely small.

The breadth of this bar was 0.92 inches; the depth 0.36 inches; and the distance between the supports 24 inches. It was soft, so as to yield easily to the file.



J. Basire sc.

	lbs.		inches.
with a load of	18.6	the depression in the middle was	0.05
	37.0	- - - - - - -	0.10
	47.0	- - - - - - -	0.127

The bar was then hardened, so that a file made no impression on any part of it, and the same loads did not produce flexures that were sensibly different from those in the soft state.

I then lowered the temper till it assumed an uniform straw colour;

	lbs.		inches.
when with a load of	47	the depression in the middle was	0.127
	85	- - - - - - -	0.230
	130	- - - - - - -	0.350
	150	- - - - - - -	0.400

The load of 150lbs. produced a permanent set of 0.012, but 130lbs. produced no sensible effect. The loading was con-

	lbs.		Inches.
tinued, and with	185	the depression in the middle was	0.50
	385	- - - - - - -	1.04

When 385lbs. had been upon the bar about a minute, it emitted a faint creaking sound, and consequently I ceased to add fresh weights; in about fourteen minutes the bar broke, exactly in the middle of the length.

On comparing the fractures of the specimens, there was no apparent difference except in colour. The grain was fine, and equal; the small sparkles of metallic lustre abundant, and equally diffused; but in the harder specimen they had a whiter ground.

From these experiments it appears that the elastic force of steel is sensibly the same in all states of temper.

The height of the modulus of elasticity, calculated by the

formula you have given in your Nat. Phil. (Vol. II. p. 48) is, according to the first experiment, - - 8,827,300 feet. And according to the second experiment 8,810,000 feet.

Now the height of the modulus, as you had determined it for steel by Experiments on Vibration, is 8,530,000 feet. (Nat. Phil. II. p. 86.) The modulus for cast steel calculated from DULEAU's experiments (Essai Théorique et Expérimental sur le Fer Forgé, p. 38) is 9,400,000 feet, and for German steel 6,600,000 feet.

The force which produces permanent alteration is to that which causes fracture in hard steel, as 350 : 580; or as 1 : 1.66 in the same steel of a straw yellow temper as 150 : 385, or as 1 : 2.56.

When the tension of the superficial particles at the strain which causes permanent alteration, is calculated by the formula given in my Essay on the Strength of Iron, p. 146, 2nd Edition, it is 45,000lbs. upon a square inch in tempered steel; and the absolute cohesion 115,000lbs. Mr. RENNIE found the direct cohesion of blistered steel to be 133,000lbs. (Philosophical Transactions for 1818.)

But in the very hard bar, the strain which produced permanent alteration was 51,000lbs. for a square inch, and the absolute cohesion only 85,000lbs.

From these comparisons I think it will appear, that in the hardening of steel, the particles are put in a state of tension among themselves, which lessens their power to resist extraneous force. The amount of this tension should be equal to the difference between the absolute cohesion in different states. Taking Mr. RENNIE's experiment as the measure of cohesion in the soft state, it will be 133,000 − 115,000 =

18,000lbs. for the tension with a straw yellow temper; and 133,000 — 85,000 = 48,000lbs. for the tension in hard steel. And if this view of the subject be correct, the phenomena of hardening may be explained in this manner, which nearly agrees with what you have observed in your Lectures I, p. 644: after a piece of steel has been raised to a proper temperature, a cooling fluid is applied capable of abstracting heat more rapidly from the surface than it can be supplied from the internal parts of the steel. Whence the contraction of the superficial parts round the central ones which are expanded by heat; and the contraction of the central parts in cooling, while they are extended into a larger space than they require at a lower temperature, produces that uniform state of tension, which diminishes so much the cohesive force in hard steel. The increase of bulk by hardening agrees with this explanation; and it leads one to expect, that any other metal might be hardened if we could find a means of abstracting heat with greater velocity than its conducting power.

I am, Sir,

Your most obedient Servant,

THOMAS TREDGOLD.

To Dr. Thomas Young,
&c. &c.

XIX. *A short Account of some Observations made with Chronometers, in two Expeditions sent out by the Admiralty, at the recommendation of the Board of Longitude, for ascertaining the Longitude of Madeira and of Falmouth. In a Letter to* THOMAS YOUNG, *M. D. For. Sec. R. S. and Secretary to the Board of Longitude. By Dr.* JOHN LEWIS TIARKS.

Read April 29, 1824.

SIR,

AGREEABLY to the wish of the Board of Longitude, communicated to me through you, I beg to transmit to you the following short statement of the results of chronometrical observations, which were made by order of that Board in the year 1822 and 1823, chiefly regarding the longitude of places in England. I have compared the results thereby obtained, with the corresponding ones contained in the Account of the Trigonometrical Survey of England, published in several volumes of the Philosophical Transactions, and have added some remarks on the method employed in calculating the results of that Survey, with a view to explain the cause of the errors in the longitude of all stations, to which it seems to have given rise. In the year 1822, being ordered to determine the difference of longitude between the island of Madeira and Falmouth, I took fifteen chronometers from Greenwich to Falmouth by sea, and after having performed a voyage to Madeira, carried them back from Falmouth to Greenwich. Both before and after each of the two voyages between Greenwich and Falmouth, the time and the rates of the

chronometers were accurately determined at Greenwich by the Astronomer Royal, and at Falmouth by myself; and applying the means of the rates of each chronometer before and after each voyage, as the rate during the voyage, the means of all results nearly agreed in giving the longitude of Pendennis Castle (the station near Falmouth) 4" (time) greater than it is laid down in the Account of the Trigonometrical Survey. Although it thus became very probable that some error had crept into the determinations deduced from the Survey, still the result of the chronometers, considering the manner in which it was obtained, could not be looked upon as completely adequate to prove even the existence, and much less the amount of an error, which was before so little expected. As this question affected however the longitude of all places in England determined in the same manner as Pendennis Castle, and likewise that of the island of Madeira, the difference of longitude of which from Falmouth had been ascertained, it was resolved to determine more accurately, by means of chronometers, the difference of longitude between Falmouth and Dover, the latter being a station in the Survey easily accessible by sea, and its difference of longitude from the former nearly the greatest possible in England, viz. more than 6°¼. In pursuance of the method adopted for this purpose, the chronometers were constantly transported from the one place to the other, as soon as the time at the former was determined with sufficient accuracy. It is clear that in this manner there may be deduced from each chronometer two sets of numbers, one for each place, representing the difference of the time of the chronometer from the mean time of the place at successive moments, and

that between any two successive terms of one set, a term corresponding in time to one of the other set may be determined by interpolation, which will represent the difference of the chronometer from the mean time of one place at the same moment at which the difference from the mean time of the other place was determined by actual observation. The difference of two such terms of both sets answering to the same moment being the difference of longitude of the two places, it follows, that the number of results thus obtained, is always equal to the sum of the numbers of terms in both sets, *minus* two, or to the number of trips from one place to the other, *minus* one. The chronometers which were employed on the occasion were carefully placed on board ship, and never removed from their places during the whole time of the expedition; and no rate was therefore used which was determined while they were in another situation; an advantage, it appears to me, of some consequence. The time was determined by numerous equal altitudes of the sun, taken with a sextant, for which two assistants carefully noted down the moments of observation, by two chronometers; the differences of which from the other chronometers were ascertained by several comparisons before and after taking the observations on shore. The number of observations being generally sufficiently great to destroy, in the mean of them, the small errors of each observation, and the rates of the chronometers nearly uniform during the time which it was necessary to stay on shore, the two chronometers usually gave nearly equal results, the mean of which was afterwards adopted in deducing the state of the other chronometers. When the advanced state of the season rendered it dangerous to ride at

anchor in Dover roads, it was thought useful to connect Portsmouth with Falmouth in the same manner. The following are the days on which the observations for ascertaining time were taken, from which the results were derived: 1823. 1. July 30, Dover. 2. August 4, Falmouth. 3. August 6, Dover. 4. August 11, Falmouth. 5. August 6, Dover. 6. August 24, Portsmouth. 7. August 30, Falmouth. 8. September 2, Portsmouth. 9. September 7, Falmouth. 10. September 11, Portsmouth. The difference of each chronometer from the mean time of the respective places being therefore known on these days, there may be employed two modes of calculating the difference for an intermediate moment, viz. the rate between every two successive differences belonging to the same place may be considered as uniform, and the intermediate term is accordingly found by simple proportion from two terms only; or the intermediate term may be derived by interpolation from all the terms, provided that in the whole interval no external cause have acted on the chronometers so as to produce a sudden change in their rates. Thus if d', d'', d''' represent the difference from the mean time of the same place at the moments t', t'', t''', this difference will be for the moment $t = \frac{(t-t'')(t-t''')}{(t'-t'')(t'-t''')} d' + \frac{(t-t')(t-t''')}{(t''-t')(t''-t''')} d'' + \frac{(t-t')(t-t'')}{(t'''-t')(t'''-t'')} d'''$. Each of the above days, except the first and last, respectively combined with two, or all the days belonging to the other place, will therefore give a result. Having thus explained the method which I have used, it will be sufficient to give the mean results of the chronometers, out of which one only was rejected on account of its irregular rate.

Difference of Longitude between Dover and Falmouth, by twenty-four Chronometers.

First Mode of Interpolation.		Second Mode of Interpolation.	
Observations.	Mean Results.	Observations.	Mean Results.
	h ′ ″		h ′ ″
2 and 1.3	0 25 28.351	2 and all Dover Obs.	0 25 28.283
3 and 2.4	28.436	3 and all Falmouth Obs. . .	28.722
4 and 3.5	29.152	4 and all Dover Obs. . . .	28.942
5 and 4.7	27.104	5 and all Falmouth Obs. . .	28.346
Mean	0 25 28.261	Mean	0 25 28.573

Difference of Longitude between Portsmouth and Falmouth, by twenty-six Chronometers.

First Mode of Interpolation.		Second Mode of Interpolation.	
Observations.	Mean Results.	Observations.	Mean Results.
	h ′ ″		h ′ ″
6 and 4.7	0 15 42.560	6 and all Falmouth Obs.	0 15 43.680
7 and 6.8	45.845	7 and all Portsmouth Obs. . .	46.362
8 and 7.9	46.710	8 and all Falmouth Obs. . .	46.731
9 and 8.10	45.534	9 and all Portsmouth Obs. .	46.622
Mean	0 15 45.162	Mean	0 15 45.849

Taking the mean of all results, we have finally for the difference of longitude between Dover and Falmouth $0^h\ 25'\ 28''.417$. The longitudes of the Survey, (applying the reduction to my station at Dover, which was nearly $0''.1$ east of that of the Survey) will give the same difference $0^h\ 25'\ 23''.5$. The difference of longitude of Portsmouth Observatory, near which I observed, and Falmouth, is by the Survey $0^h\ 15'\ 43''.0$. Although the observations from which the above given results for the same difference of longitude are derived, were made under less favourable circumstances and at greater intervals, (especially the first) and therefore do not agree so

well among themselves, still they prove that the result of the Survey is about 8″ too small; or that it is nearly in the same proportion too small, in which the difference of longitude between Dover and Falmouth, and likewise by the observations of the year 1822, that between Greenwich and Falmouth were found to be deficient. We may therefore safely infer, that it is a general and proportionate defect of all longitudes deduced from the Survey, and not the erroneous longitude of any particular station, which has caused the disagreement between the results of the chronometers and of the Survey. Supposing the final result above found to be correct, the increase of the longitude of the Survey $=25' 23''.5$, is $4''.92$, and at this rate all the longitudes contained in the Account of the Survey must therefore be increased. Applying this correction, we shall have,

the longitude of Dover (at the Station of the Survey) . .	$0^h\ 5'\ 17''.52$ E
Portsmouth (at the Station of the Observatory) .	4 24 .75 W
Pendennis Castle (at the Station of the Flag Staff)	20 10 .81
In the year 1822 I found the latter, by going from Greenwich to Falmouth .	20 11 .49
and by returning from Falmouth to Greenwich	20 11 13

The difference of longitude between Falmouth and Madeira was found, by the mean of the results of seventeen chronometers, to be $0^h\ 47' 28''.21$. The extremes of these results differ indeed $20''$, but as 9 of them, the mean of which is $0^h\ 47' 28''.23$, do not differ more than $3''.71$ from one another, it is to be presumed, that the above given result is not far from the truth. The station at Madeira (the garden of the British Consul in the town of Funchal) is therefore in longitude $1^h\ 7' 39''.02$ W of Greenwich.

Having now fully proved the errors of the longitudes of the Survey, and likewise shown in what manner they are to be corrected, I conceive it to be of some interest to investigate the cause of the mistake into which those distinguished men have fallen, who conducted this great national undertaking with so much ability and perseverance. The safest and most obvious method of reducing the results of a survey of a country with respect to longitude and latitude, would be (if practicable) to determine astronomically, in the country itself, arcs both of the meridian and of a parallel. The spheroid nearest approaching to the figure of the earth for that country, would be easily deduced from these measurements, and all the reductions would be perfectly correct; but as the determination of an arc of longitude is exceedingly difficult, this method has hardly ever been practised. An arc of the meridian is more easily determined, and the reductions with respect to latitude are readily obtained. In order to find, however, in the most correct manner the arcs of parallels of latitude, it is necessary to combine an arc of a meridian measured in the country, with another measured in the same hemisphere as different from it as possible, in order to determine the eccentricity of the meridians, and the dimensions of the corresponding spheroid. The measurements near the Equator and near the Pole may thus be combined with the arcs measured in several parts of Europe, and the errors in the longitude, arising from the adoption of a spheroid thus determined, will be very small. In this manner BOUGUER's degree near the Equator and the degree of the meridian in the middle between Greenwich and Paris, as they are given in the account of the Survey, would have led to a spheroid of the compression $\frac{1}{398}$,

and all the reductions would have been nearly correct both in latitude and longitude. But instead of proceeding in this manner, it seems to have been the intention to determine, independently of any hypothesis respecting the figure of the earth, from a line, the length of which was ascertained by geodetical measurement, the length of a degree perpendicular to the meridian, in the same latitude in which the length of the arcs of the meridian was known by the distance of the parallels of Greenwich and Paris; an arc of the meridian, and one perpendicular to it, being sufficient to determine the dimensions of the terrestrial spheroid. The line chosen for this purpose is the distance between Dunnose and Beachey Head (DB); its length was ascertained in various ways, all of which gave results nearly approaching one another. As the inclinations of the meridians of Beachy Head and Dunnose (to the line DB) had been observed, and the latitude of the two spots were supposed to be known, both places having been connected by the Survey with Greenwich; it is clear, that in the spheroidical triangle, North Pole, Beachy Head, Dunnose (PBD), the two angles B and D, the sides (PB and PD) and besides the length of the line BD are known. By resolving the spheroidical triangle PBD, the angle P (difference of longitude of B and D) is ascertained. From this and the line DB, the length of the degree perpendicular to the meridian in latitude $50° 41'$ nearly, the middle point between Beachey Head and Dunnose is determined; and thence a spheroid, with a compression $\frac{1}{149}$ is found, on which all the reductions have been founded. The latitude of Dunnose, determined by its distance from the parallel of Greenwich, is $50° 37' 7''.8$, or diminished by $1''.99$ (the correction of the lati-

tude of Greenwich Observatory, which Captain KATER applies as the result of the latest observations, and the use of the French table of refraction 50° 37′ 5″.31. Captain KATER finds this latitude by his observations with the repeating circle 50° 37′ 5.″27, differing only 0″04 from the other. The latitude of Dunnose being therefore correctly deduced by geodetical operations from the latitude of Greenwich, it is to be supposed that the latitude of Beachy Head, the more northern point, deduced by the same operations, has been equally well determined, and at least that there is no considerable error in the difference of latitude of the two places as laid down in the Survey.

In order to understand the method which I have shortly described above, it is to be observed, that the spheroidical triangle PBD could not be resolved from the angles B and D and one of the sides PB and PD only, without assuming a certain ellipticity; but from the two angles and the two sides, the ellipticity may be determined directly; and from this, and the length of the arc of the meridian, the dimensions of the terrestrial spheroid may be found. Introducing therefore the two angles and the two sides into the calculation, as is done in the Survey, is assuming that spheroid for the basis of the calculation, which has its compression determined by the relation between the two angles, the two sides, and the eccentricity of the meridians. For let the eccentrity of the meridian be e, the latititude of Beachy Head φ', Dunnose φ, the angles $\left\{\begin{array}{l} B=\alpha' \\ D=\alpha \end{array}\right\}$

and we have by the property of the geodetical line (the shortest line between two points on the terrestrial spheroid) $\frac{\sin.\,\alpha\,.\,\cos.\,\varphi}{\sqrt{(1-e^2\sin.\,\varphi^2)}} = \frac{\sin.\,\alpha'\,\cos.\,\varphi'}{\sqrt{(1-e^2\sin.\,\varphi'^2)}}$, or if ξ', ξ and ψ be determined

by the following equations, tang. $\xi' = \frac{\sin. \alpha^2}{\cos. \varphi'^2}$, tang. $\xi = \frac{\sin. \alpha'^2}{\cos. \varphi^2}$, tang. $\psi^2 = \frac{\sin. (\xi' - \xi)}{\cos. \xi' . \cos. \xi . \sin. (\alpha' + \alpha) . \sin. (\alpha' - \alpha)}$, we have $e = \sin. \psi$, and the compression $= 2 . \sin. \frac{1}{2} \psi^2$.

From e and an arc of the meridian the dimensions of the spheroid are readily found; the angle P, and the length of the geodetical line BD, may likewise be determined. The geodetical line BD, which is used in the Survey to find the degree perpendicular to the meridian, furnishes therefore no new datum for determining the dimensions of the parallels; and this line will vary very little for different values of e, provided the values of α and α' and one of the latitudes be the same, while the other latitude is determined by the above equation. But the value of P will be different for every value of e; and the same geodetical line will therefore correspond to different values of the difference of longitude according to the different compressions which are assumed. It follows therefore from all this, that the dimensions of the parallels have entirely been derived from the arc of the meridian in latitude 50° 41′, and the compression resulting from the values of α, α', φ, φ'. The angle α was observed $= 81° 56' 53''$. $\alpha' = 96° 55' 58''$; φ is according to Captain KATER $= 50° 37' 5''.27$, and the value of φ', resulting from the addition of the difference of latitude of B and D to φ, is $= 50° 44' 21''.67$. This value gives, by the above formula, the compression $\frac{1}{150.5}$, nearly the same as found in the Survey. For the compression $\frac{1}{335}$, the value of φ' would be $50° 44' 20''.35$ for; $\frac{1}{310}$ $\varphi' = 50° 44' 20''.42$; for $\frac{1}{230}$ $\varphi' = 50° 44' 20''.79$. From what was remarked before, it is clear that α and α' being the same, no

compression considerably differing from $\frac{1}{150}$ can be admitted. In order to produce an ellipticity $= \frac{1}{310}$, it would be necessary to diminish both a and a' by $5''.5$. Although it would be difficult to assign the limits of the errors that may have been committed in determining a' and a, still it is not very likely that so great an error should have been made in both places. It is therefore likely that the meridians have, in that part of the country, a greater ellipticity than the whole earth, which would not be surprising, as some of the arcs measured in France appear to indicate even a compression $= \frac{1}{175}$.

From the foregoing observations we may now conclude, that the longitudes laid down in the account of the Survey will deviate from the truth, in the same proportion in which the parallels of latitude on a spheroid, having the degree of the meridian in latitude 50° 41′ equal to that of the earth, and the ellipticity $\frac{1}{149}$ differ from those of the terrestrial spheroid, the compression of which is nearly $\frac{1}{310}$. The following comparisons will further illustrate the subject. If the radius of the Equator be = 3486908 fathoms, and the semi-axis of the earth = 3475560 fathoms, which is nearly the result of the measurements in France, and Bouguer's in Peru, and corresponds to the compression $\frac{1}{310}$, the length of the degree perpendicular to the meridian in latitude 50° 41′ will be 60975.7 fathoms. For the spheroid adopted in the Survey, that degree is found 61,182 fathoms. The ratio of these numbers is 296 : 297, and the correction of the longitudes would be $\frac{1}{296}$; the same correction is, by the chronometrical

observation, $\frac{1}{309}$. The length of the geodetical line BD, supposing the difference of longitude as determined in the account of the Survey, viz. 1° 26′ 47″.93, would be 338231 feet; whereas it was found to be 339397.6 feet; but if the longitude be increased in the ratio determined by the chronometers, the line will be 339334 feet, which is only 63.6 feet short of the measurement. The spheroid resulting from the compression which would make the difference of longitude of B and D = 1° 27′ 4″.75 (as it ought to be according to the results of the chronometers), and from the degree of the meridian in latitude 50° 41′, viz. 60851 fathoms, would have these dimensions: radius of the Equator = 3487907 fathoms; semi-axis = 3476687 fathoms; compression $\frac{1}{314}$. The results of the chronometrical observations are therefore as much as could be expected in accordance with the correct determinations of the figure of the earth.

I am, Sir,

your most humble and obedient Servant,

J. L. Tiarks.

To Dr. Young,
Secretary of the Board of Longitude.

XX. *Of the effects of the density of air on the rates of chronometers. By* George Harvey, *F. R. S. E. &c. Communicated by* Davies Gilbert, *Esq. V. P. R. S.*

Read May 13, 1824.

Among the different sources of error to which chronometers are commonly considered to be liable, the effects of the variable density of the medium in which the balance performs its vibrations has, in some degree, been overlooked. That changes in the density of the medium, produce however a sensible influence on the rate of a delicate time-keeper will, I hope, clearly and satisfactorily appear, from the detail of the experiments, now respectfully submitted to the Royal Society.

The investigation of the subject has been undertaken in the four following points of view:

First, by subjecting different chronometers to a *less* pressure than that afforded by the ordinary state of the amosphere at the level of the ocean.

Secondly, by submitting them to a *greater* pressure than that afforded by the atmosphere under the same conditions.

Thirdly, by removing chronometers from condensed into rarified air, and *vice versa*.

And *fourthly*, to determine how far the rates of chronometers are affected by the ordinary aberrations of atmopheric pressure at the level of the sea.

1. To estimate the effects produced by the first of these

conditions, the chronometers were placed beneath the capacious receiver of a large double barreled air pump, the pressure being indicated by an excellent mercurial guage.

To prevent any irregular effects from the unequal action of terrestrial magnetism, the position of each chronometer, with respect to the meridian, was preserved *constant* during the whole course of experiments.*

The first chronometer selected was an eight day one, of the box kind; and which, for the purpose of farther reference, I shall distinguish by the letter A. Its rate for ten days previous to the experiments was steady and uniform, amounting to — 3″.1, the mean pressure of the atmosphere being 30.1 inches; but when placed beneath the receiver of the air pump, under a constant pressure of 20 inches of the mercurial column, the mean of four days observation gave an equally steady rate of — 1″.3, the chronometer having gained 1″.8, by diminishing the density of the air in the ratio of 3 to 2. By farther exhausting the air, so as to make its density correspond with ten inches of the column of quicksilver, the mean of a like number of days gave a remarkably steady rate of + 1″.7, being an increase of 3″.0 on the former rate; and by continuing the exhaustion until the mercurial column sunk to an inch, the average of the same number of days produced a rate of + 6″.6, being a farther increase of 4″.9. Thus, an alteration of + 9″.7 in the rate of the chronometer was prodused by diminishing the density of the air in the ratio of 30 to 1; and on removing it from the

* That chronometers, having their balances magnetic, experience alterations from being placed in different positions with respect to the magnetic meridian, is no longer doubtful.

highly rarefied air in which the last experiment placed it, to the full pressure (29.85 inches) of the atmosphere, the mean of ten days observations gave an average rate of $-6''.3$; the time-keeper having altered its detached rate $-3''.2$, in consequence of the experiment. These alterations of rate were produced in each case immediately after the density of the air was changed.

Adopting the last mentioned rate as a standard, the time-keeper was next submitted, during four days, to a pressure denoted by 25 inches of the mercurial column, when its rate was immediately changed from $-6''.3$ to $-2''.2$, being an increment of $4''.1$. By farther diminishing the density to a quantity represented by 10 inches of quicksilver, the average of a like period gave a rate of $+2''.7$, being another increment of $4''.9$; and on again exhausting the air till the mercury was depressed to 5 inches, the mean of four days produced a rate of $+4''.6$, being a third increment of $1''.9$; and by restoring the chronometer to the ordinary pressure of the atmosphere, an immediate change in its rate took place; the mean of ten days producing an average of $-5''.9$, the close approximation of which to its former detached rate being remarkable.

The results of the preceding experiments are entered for the purpose of a more convenient reference in the succeeding tables; together with the average heights of the barometer during the times the detached rates were determined; and also the mean temperatures through the entire series of observations.

Eight-day Box-Chronometer A.			
Mean Temp.	Pressure.	Number of days.	Mean daily rate.
First set of Experiments.			
58°	detached 30 in.	5	− 3″.1
59	20 in.	4	− 1″.3
59	10 in.	4	+ 1″.7
59	1 in.	4	+ 6″.6
59	detached 29.85 in.	10	− 6″.3
Second set of Experiments.			
59	detached 29.85	10	− 6″.3
57	25 in.		− 2″.2
58	10 in.	4	+ 2″.7
58	5 in.	4	+ 4″.6
57	detached 30.02 in.	10	− 5″.9

These remarkable changes having taken place in the daily rate of a chronometer, known under the ordinary circumstances of atmospheric pressure to preserve a steady uniformity of rate, I was induced to employ, in the next place, three good pocket chronometers, whose detached rates are

recorded in the first horizontal line of the succeeding table. The time-keepers were placed at the same instant beneath the receiver, and the air exhausted until the mercurial column sunk to half an inch; when the singularly large increments entered in the second line were produced. In the third horizontal line of the table, the rates of the chronometers are given after they were restored to the full pressure of the atmosphere; and it is remarkable, how closely they approximate to the primitive rates, the greatest aberration being only 1″.3, in chronometer C. Nor ought the close approximation also of the large increments produced by the exhaustion to a state of equality to be entirely overlooked. The chronometers were taken each day from beneath the receiver for the purpose of comparison, and being wound up; a circumstance only necessary once a week, with the chronometer A.

Experiments with Pocket Chronometers, B, C, D.					
Mean Temp.	Pressure.	Number of Days.	Mean daily rate of B.	Mean daily rate of C.	Mean daily rate of D.
58°	detached 30.04 in.	6	+ 4″.7	+ 0″.3	+ 3″.6
58°	½ inch	6	+ 23″.5	+ 18″.6	+ 23″.5
56°	detached 30.01 in.	6	+ 5″.0	+ 1″.6	+ 2″.4

The preceding changes having been produced by diminishing the density of the air in the ratio of 60 to 1; an

attempt was next made, to ascertain the effect of a more gradual diminution thereof, on the same chronometers. The results are recorded in the following table:

Experiments with Pocket Chronometers, B, C, D.

Mean Temp.	Pressure.	Number of Days.	Mean daily rate of B.	Mean daily rate of C.	Mean daily rate of D.
56°	detached 30.01 in.	6	+ 5″.0	+ 1″.6	+ 2″.4
58°	20 in.	4	+ 9″.3	+ 6″.2	+ 9″.3
58°	10 in.	4	+ 18″.5	+ 11″.0	+ 17″.6
57°	detached 29.76 in.	4	+ 6″.6	+ 2″.1	+ 3″.5

A still more gradual decrease of density was preserved in another set of experiments, the results of which are recorded in the next table.

Experiments with Pocket Chronometers, B, C, D.

Mean Temp.	Pressure.	Number of Days.	Mean daily rate of B.	Mean daily rate of C.	Mean daily rate of D.
57°	detached 29.76 in.	4	+ 6″.6	+ 2″.1	+ 3″.5
59°	25 in.	4	+ 10″.4	+ 3″.6	+ 5″.7
59°	15 in.	4	+ 12″.2	+ 7″.6	+ 12′.1
60°	5 in.	4	+ 19″.2	+ 11″.4	+ 20″.6
60°	detached 29.84 in.	4	+ 6″.0	+ 0″.6	+ 4″.2

It is remarkable, that the increments resulting from the unequal detached rates of the chronometers B and D, should in two instances, viz. those corresponding to half an inch, and 20 inches of quicksilver, be precisely the same; and in three other cases, viz. 15 inches, 10 inches, and 5 inches respectively, very nearly so; the only considerable deviation being in the rate of the time-keeper D, when under a pressure corresponding to 25 inches of quicksilver. This chronometer however recovered itself when under a pressure of 15 inches.

In another set of experiments, and of which the results are found in the succeeding table, the density of the air under the receiver was uniformly diminished by decrements represented by two inches of quicksilver, and which was accompanied by changes in the rates of the two chronometers employed, (abstracting the occasional aberrations displayed by most time-keepers) increasing proportionally as the density of the air was decreased. From the nearly equal uniformity of temperature also that prevailed during each set of experiments, and from the positions of the chronometers with respect to the magnetic meridian having likewise been preserved constant, there can be no doubt but the different alterations of rate are due to alterations of pressure.

Experiments with Pocket Chronometers C, D.

Mean Temp.	Pressure.	Number of Days.	Mean daily rate of C.	Mean daily rate of D.
° 46	Detached 29.1 in.	4	" + 2.5	" + 4.0
46	27	4	+ 3.0	+ 5.2
47	25	4	+ 3.4	+ 6.1
46	23	4	+ 4.7	+ 7.2
45	21	3	+ 6.4	+ 9.4
42	19	4	+ 7.6	+ 11.3
43	17	3	+ 10.5	+ 13.4
44	15	4	+ 11.7	+ 14.4
45	13	4	+ 13.4	+ 15.8
49	11	4	+ 14.5	+ 17.1
48	9	4	+ 16.0	+ 18.2
48	7	3	+ 19.0	+ 20.4
50	5	4	+ 18.6	+ 22.1
49	3	4	+ 19.9	—

The next set of experiments was performed with the eight day chronometer E, possessing the very excellent detached rate recorded in the first of the following columns. In the second column of the same table is also entered the daily rate of the same chronometer when placed in air, diminished in density in the ratio of 60 to 1; and in the third column is its rate, when afterwards restored to the full pressure of the atmosphere. The mean temperature of the first period was 56°, of the second 58°, and of the third 57°.

Experiments with an Eight-day box Chronometer, E.		
Mean Pressure prior to the Experiment 29.95 inches.	Pressure during the Experiment half inch.	Mean Pressure after the Experiment 29.90 inches.
Daily Rates.	Daily Rates.	Daily Rates.
″ — 5.4	″ — 7.5	″ — 6.3
— 5.1	— 8.7	— 7.1
— 5.6	— 9.5	— 5.8
— 5.8	— 9.2	— 5.8
— 5.7	— 9.2	— 5.1
— 5.6	— 9.0	— 5.9
— 5.2	— 9.4	— 6.0
— 5.1	— 9.4	— 6.6
— 5.5	— 9.7	— 5.0
— 5.6	— 9.9	— 5.7
— 5.4	— 9.9	— 5.7
Mean daily rate. — 5.45	Mean daily rate. — 9.32	Mean daily rate. — 5.91

The preceding chronometer having therefore *decreased* its rate in consequence of a *diminution* of pressure, contrary to the uniform character of the former experiments, I was induced to extend my field of investigation; and for that purpose obtained from my naval friends several more chronometers. And in order to render the experiments of greater utility in their ultimate applications, I decided on submitting the time-keepers to the influence of air corres-

ponding in density to the mean state of the atmosphere, at some remarkable places, situated at considerable elevations above the level of the sea.

For this purpose several chronometers were supposed to be carefully transported from London to Geneva, every circumstance relating to temperature and magnetism remaining constant; and from the succeeding table it will appear, that an alteration in the rate of the time keeper will be the necessary result. In this experiment two box chronometers, and three of the pocket kind, had their rates carefully determined for five days, under a mean atmospheric pressure of 29.86 inches, and an average temperature of 50°; and for a like number of days in air corresponding in density to 28.6 inches, being the mean atmospheric pressure at Geneva, the average temperature being 48°. The following alterations of rate resulted; and which clearly demonstrated that a constant difference of 1,26 inches in the mercurial columns, was capable of affecting very sensibly the rates of the chronometers employed.

Experiment for Geneva, elevated 201 fathoms above the level of the Sea.								
Place.	Mean Temperature during the Experiment.	Mean Pressure.	Number of days.	Mean daily rate of A.	Mean daily rate of C.	Mean daily rate of F.	Mean daily rate of G.	Mean daily rate of H.
London,	50°	Detached 29.86 in.	5	″ −3.3	″ +0.6	″ +2.0	″ −7.1	″ +7.9
Geneva,	48	28.60 in.	5	−2.7	+1.2	+0.5	−3.1	+9.7

Of the above alterations of rate it will be perceived, that the box chronometer F *lost* by being submitted to a *diminished* pressure; but that the others *gained.*

These results having been produced by air corresponding in density to its mean state at 201 fathoms above the level of the sea, another set of experiments was instituted on the supposition that a chronometer was removed from the shores of the Mediterranean to the lofty plains of La Mancha and the Castiles, where the mean barometric pressure is denoted by 27.81 inches. The following table contains the results:

Experiment for Madrid, elevated 329 fathoms above the level of the Sea.

Place.	Mean Temp. during the Experiment.	Mean Pressure.	Number of Days.	Mean daily rate of A.	Mean daily rate of F.	Mean daily rate of H.	Mean daily rate of I.
London	52°	detached 29.86 in.	4	−4″.5	+3″.1	+7″.4	−17″.4
Madrid	53	27.81	5	−2.4	+0.8	+9.6	−13.1

Three of the chronometers employed in the two preceding sets of experiments, viz., A, F, H, were the same, and which therefore enables us to perceive, that a difference of $\frac{8}{10}$ths of an inch in the barometric column produces an alteration of rate. The changes also are more considerable in proportion as the difference in the altitudes of the mercurial column is increased, as is proved in the succeeding table.

Difference in the Altitudes of the Mercurial Columns.	Increment communicated to the Chronometer A.	Increment communicated to the Chronometer H.	Decrement communicated to the Chronometer F.
1.26 in.	+ 0″.6	+ 1″.8	− 1′.5
.205 in.	+ 2″.1	+ 2.″2	− 2″.3

The mean also of five days observations with chronometer H, in a medium corresponding in density to 25.95 inches of mercury, being the average state of the barometer at the Palace of Ildefonso, 630 fathoms above the ocean, produced an increment in its rate amounting to 3″.0; and which taken in conjunction with the results of the same chronometer recorded in the preceding table, prove that it's rate varied nearly with the density of the medium in which it was placed. This chronometer is one, on which in many delicate inquiries I particularly depended.

Although it appears from the preceding experiments, that the alterations of rate in the *same* chronometer depend on the density of the medium in which it is placed; yet the magnitudes of the changes in *different* time-keepers appear o be very unequal, arising no doubt from peculiarities of construction. Thus, a set of experiments was undertaken with two good box chronometers, in order to discover what changes would take place in their rates if they were transported from Vera Cruz, on the shores of the Pacific Ocean, to the Table Land of Mexico, where the mean atmospheric pressure is denoted by 23 inches of the barometer. The result proved, that the change in the rate of the chronometer represented by K, amounted only to + 1″.9, and in the time-keeper L, to — 5″.0; the former having *gained* by the *diminished* pressure, and the latter *lost*.

Experiment for Mexico, elevated 1253 fathoms above the level of the Sea.					
Place.	Mean Temp. during the Experiment.	Mean Pressure.	Number of Days.	Daily rate of K.	Daily rate of L.
Vera Cruz	54°	30 in.	5	— 5″.9	+ 4″.8
Mexico	54°	23 in.	4	— 4″.0	— 0″.2

If we again conceive a set of chronometers removed from the level of the ocean to Santa Fé de Bogota, or to the still loftier city of Quito, at which latter place the mean altitude of the mercurial column is 21 inches, the alterations of rate, recorded in the next table, will be found to result from the diminished pressure.

Experiment for Quito, elevated 1603 fathoms above the level of the Sea.					
Place.	Mean. Temp. during the Experiment.	Mean Pressure.	Number of Days.	Daily rate of H.	Daily rate of C.
Level of the ocean.	48°	30 in.	4	+ 8″.0	+ 3″.0
Quito	47°	21 in.	5	+ 14″.2	+ 6″.2

Four chronometers were afterwards placed in air corresponding in density to 15 inches of mercury, being nearly the altitude of the barometer on Chimborazo, when the results recorded in the following table were produced.

Experiment for the summit of Chimborazo, elevated 3578 fathoms above the level of the Sea.							
Place.	Mean Temp. during the Experiment.	Mean Pressure.	Number of days.	Daily rate of A.	Daily rate of C.	Daily rate of F.	Daily rate of H.
Level of the Sea	52°	30 in.	4	− 1″.9	+ 2″.6	+ 2″.0	+ 7″.9
Summit of Chimborazo	51°	15 in.	4	+ 4″.2	+ 9″.6	− 3″.3	+ 17″.0

Three of the chronometers used in the last experiment, viz. A, F, and H, were the same as those employed in the experiments for Geneva and Madrid; and from which it will be perceived, that even under the influence of a much less pressure, the alterations of rate partook of the same character; in one instance (F) *losing* from *diminished* pressure, and in the other cases (A, H,) *gaining*. The changes in the last experiment are more considerable, in consequence of the greater rarity of the air.

As a final experiment on this part of the subject, the two excellent pocket chronometers (C, H,) employed in the last experiment, were placed beneath the receiver, and the air exhausted until the mercurial guage sunk to 12.95 inches, being the elevation of the barometric column observed by Gay Lussac in his magnificent aerostatic ascent. The consequent alterations of rate were respectively + 13″.2, and + 19″.2.

From the preceding experiments it may therefore be inferred, that a chronometer constructed in air, corresponding in density to its mean state at the level of the ocean, will undergo alterations of rate, when removed into a region

where the average density is sensibly diminished. In a subsequent part of the paper it will also appear, that the uncertain fluctuations of the barometer at the level of the sea, produces in many cases minute, but sensible changes of rate.

2. The effects recorded in the preceding section of the paper having been produced by a *diminution* of atmospheric pressure, it was conceived, that results entirely the reverse would arise from an increase thereof; that is, if a chronometer *gained* by being placed in air of a *less* density than that afforded by the ordinary state of the atmosphere, it ought to *lose*, by being subjected to air of a *greater*. Accordingly, by introducing different time-keepers into a condensing engine, furnished with an appropriate mercurial guage, the *opposite results* here alluded to actually took place.

Not knowing the effects likely to be produced on so delicate a machine as a chronometer, by placing it in air condensed to any considerable degree, the time-keeper H, employed in the former investigations, was first placed in air of a density corresponding to 34 inches of mercury. The result was an alteration of the mean detached rate of the chronometer from + 6''.9 to + 5''.3. The chronometer was necessarily removed from the condensing machine for a few minutes each day, for the purpose of comparison, and being wound up.

On restoring the time-keeper to the ordinary pressure of the atmosphere, its rate returned to + 6''.5, agreeing within 0''.4 of its former detached rate. In a second experiment, it was subjected to air denoted in density by 38 inches of quicksilver, when its rate altered from + 6.''5 to + 2''.3 ; and on

being again detached, its rate was restored to + 6″.6, agreeing within 0.″1 of its former detached rate. These mean rates were determined in each case for five days. Hence, by contrasting the results obtained under the receiver of the air pump with those produced in the condensing engine, it will appear, that the chronometer *gained* in each experiment by *diminishing* the density of the air, and, on the contrary, *lost*, by *increasing* it.

In the next experiment the box chronometer M was placed in the condensing engine, under pressures corresponding to the mercurial columns recorded in the following table; and from which resulted an alteration of rate from — 6″.9 to — 16″.1.

Experiments with Box Chronometer M.			
Mean Temp.	Mean Pressure.	Number of days.	Mean daily rate of M.
° 49	30 in.	5	″ — 6.9
48	36 in.	5	— 8.3
48	42 in.	6	— 9.5
47	48 in.	5	— 10.8
48	54 in.	6	— 16.1

This chronometer, when placed under the receiver of the air pump in air of a less density than the ordinary state of the

atmosphere, always received *increments* to its rate; and from its having *lost* through the whole of its experiments relating to the condensed air, the same inference may be deduced from it as from the preceding chronometer. The decrements in the above table, it will be remarked, are very nearly uniform, from 30 inches to 48 inches; but in the transition from 48 inches to 54 inches, the change is considerable. It is also worthy of observation, that this time-keeper, when finally detached for twenty days, preserved a rate of nearly $-15''.0$; the dense air to which it had been so long exposed having materially augmented its losing rate, and apparently, communicated to it a permanent character.

The third set of experiments was with the pocket chronometers B and C, employed in the inquiries with the air pump, and the results of which are entered in the succeeding table.

Experiments with the Pocket Chronometers B, C.				
Mean Temp.	Mean Pressure.	Number of days.	Mean daily rate of B.	Mean daily rate of C.
47°	30 in.	6	$+4''.3$	$+1''.2$
47	45 in.	5	-5.2	-4.4
46	60 in.	5	-9.7	-8.2
45	75 in.	5	-17.2	-9.5
46	30 in.	5	$+4.5$	$+0.6$

The rates recorded in the above table, it will be perceived,

are *decrements*; whereas, in the experiments performed in the rarified air with the same chronometers, they were uniformly found to be *increments*; and hence, the results agree precisely with those recorded respecting the time-keepers H and M. The almost perfect restoration of the detached rates, after the great changes produced by so considerable an augmentation of density as that corresponding to the mercurial column of 75 inches, is a very remarkable feature of the table.

3. To obtain alterations of rate of the most striking and remarkable kind, the effects of suddenly removing chronometers from condensed into rarefied air, and *vice versa*, were estimated by a series of careful experiments.

A box chronometer, N, having its detached rate exactly coinciding with mean time when the barometric column was 29.95 inches, on being placed in air of a double density altered its rate to $-8''.6$; and when afterwards placed under the receiver in air corresponding in density to an inch of mercury, the daily average became $+10''.7$; the difference in the densities having produced an alteration of $19''.3$ in the rate. The observations were continued for each experiment five days; and the changes in the rates were produced *immediately* after the time-keeper was removed from one condition to the other. In another experiment with the same chronometer, the mean of six days, under a pressure denoted by 15 inches of quicksilver, gave a result of $+4''.1$; but on placing it in air, having three times the mean density of the atmosphere, the average of the same number of days was $-16''.7$. Hence it appears, that by removing the time-

keeper from condensed into rarefied air, it *gained*; and conversely, from rarefied into condensed air, it *lost*.

In a third experiment, the pocket chronometer O, gave a mean rate of — 6″.5 when placed in air of a density denoted by 26 inches of the mercurial column; but when placed in the condensing engine, in air of a density denoted by 45 inches of quicksilver, the rate changed to — 2″.7; and on afterwards restoring it to the ordinary pressure of the atmosphere during four days, the average rate became — 7″.7. The character of this chronometer, during many preceding experiments, was to *gain* with *greater* pressure, and to *lose* with *less*; and the preceding experiment perfectly accords with the same results.

In a fourth experiment, the pocket chronometer C was employed, having a mean rate of + 1″.2 when placed for six days under the receiver in air corresponding in density to 28 inches of mercury; but on introducing the time-keeper into the condensing engine, in air of a density denoted by 60 inches of quicksilver, the rate immediately declined to — 8″.2. In a converse experiment with the same chronometer, the rate was first determined in the condenser when the density corresponded to 36 inches of mercury, the average being + 0″.7; but on removing it into the receiver, and exhausting the air until the mercurial gauge sunk to 27 inches; the mean of four days observations gave an augmented rate of + 3″.9; and on restoring the chronometer to air of the ordinary density, its mean rate became + 2″.6, agreeing within half a second of its original detached rate. This timekeeper, in many antecedent experiments, had always been

found to *gain* with *less* pressure, and *lose* with *more*; and the tenor of the last experiments confirms the same law.

The preceding results for the chronometers C and N, may be conveniently arranged in the following tables.

Effects on the Chronometers C and N, when removed from condensed into rarefied air.							
Mean Temp.	Mean Pressure.	Number of days.	Mean daily rate of C.	Mean Temp.	Mean Pressure.	Number of days.	Mean daily rate of N.
50°	36 in.	6	+ 0″.7	47°	60 in.	5	− 8″.6
48°	27 in.	4	+ 3″.9	46°	1 in.	5	+ 10″.7

Effects on the Chronometers C and N, when removed from rarefied into condensed air.							
Mean Temp.	Mean Pressure.	Number of days.	Mean daily rate of C.	Mean Temp.	Mean Pressure.	Number of days.	Mean daily rate of N.
49°	28 in.	6	+ 1″.2	46°	15 in.	6	+ 4″.1
51°	60 in.	5	− 8″.2	45°	90 in.	6	+ 16″.7

An opportunity in another experiment was taken, when the chronometers C and D had been under the diminished pressure of 23 inches for five days, to remove them for a like period into the condensing engine, containing air of double the mean density of the atmosphere. The result of this application was, that the former rate became a decrement of

9″.5, and the latter a rate of a similar kind of 10″.4. Knowing the merits of these chronometers, I ventured to predict, that if they were removed from the condensed air into an atmosphere corresponding in density to 21 inches of quicksilver, the transition would produce rates *greater* than those corresponding to 23 inches, in consequence of the time-keeper being placed by such an experiment in air of a less density than that corresponding to the first experiment. The result verified the conjecture; the average rate of the time-keeper C having become + 5″.0, and that of Chronometer D + 12″.0. These interesting results are recorded in the next table.

Experiments with the chronometers C and D, when removed from rarefied into condensed air, and *vice versa*.

Mean Temp.	Mean Pressure.	Number of Days.	Mean daily rate of C.	Mean daily rate of D.
47°	23 in.	5	+ 2′5.	+ 9″.4
49°	60 in.	5	− 7″.0	− 1″.0
48°	21 in.	5	+ 5″.0	+ 12″.0

By contrasting also the rates of the chronometers B and C, when subjected, as in two of the preceding experiments, to air corresponding in density respectively to half an inch, and 75 inches of quicksilver; it will be perceived, that the effect of these opposite densities was to occasion an alteration of 40″.7 in the rate of the time-keeper B, and of 28″.1 in that of C. These results are recorded in the next table.

Experiments with the Chronometers B and C, when removed from rarefied into condensed air.		
Pressure.	Mean daily rate of B.	Mean daily rate of C.
$\frac{1}{2}$ in.	+ 23″.5	+ 18″.6
75 in.	— 17″.2	— 9″.5

4. It becomes now an interesting enquiry, to consider how far the ordinary changes in the density of the air may be likely to exercise an influence on the rate of a chronometer. The range of the mercurial column in London may, on an average, be estimated at $2\frac{1}{4}$ inches; and there can be no doubt but the difference produced in the density of the air by such a range must, if the transition be at all sudden, and the difference of density constant for twenty-four hours, or even less, be sufficiently considerable to affect the majority of chronometers. A great difference, however, appears to exist in this respect among time-keepers. The change of density, that in one machine of this kind would occasion an alteration or rate amounting to several seconds, in another, would scarcely produce any sensible effects; and I have found, during the whole of these experiments, a considerable difference in this particular between pocket and box chronometers; the former being most readily affected by alterations of atmospheric density.

In the following tables are recorded the results of different experiments, instituted with a view of determining the

alterations of rate occasioned by small depressions of the mercurial column below the average state of the barometer for some days previous to each experiment.

Changes produced in the rates of the Chronometers H, O, P, Q, in consequence of a diminution of pressure denoted by one inch of mercury.

Mean Temp.	Mean Pressure.	Mean daily rate of H.	Mean Temp.	Mean Pressure.	Mean daily rate of O.	Mean Temp.	Mean Pressure.	Mean daily rate of P.	Mean Temp.	Mean Pressure.	Mean daily rate of Q.
47°	30.3	+8″.5	47°	30.1	+8″.0	45°	30.1	−7″.0	49°	29.9	+4″.2
46	29.3	+9.3	48	29.1	+8.7	46	29.1	−7.9	50	28.9	+3.4

The chronometer H was employed in many of the former experiments, and it will be perceived that it constantly *gained* with *less* pressure. In the investigations instituted for Geneva and Madrid, for example, the increments communicated to its rate were respectively 1″.8, and 2″.2; and in the present instance an abberration of the same kind was produced, amounting to 0″.8; the increment being smaller than either of the preceding, in consequence of a less diminution of pressure. The close approach to numerical equality also in the changes of rates recorded in the preceding table, is not unworthy of notice, the differences being respectively − 0″.8, + 0″.7, − 0″.9, and + 0″.8. The time devoted to each set of experiments was three days.

In another set of experiments, the pocket chronometer D, and the box chronometer F, were again resorted to, and placed under the different pressures recorded in the succeeding table. The mean temperatures during the several periods of observation underwent no very considerable

change; and from the uniformly decreasing nature of the rates it is fair to infer, that the alterations resulted from diminished pressure alone, and that the successive inches of quicksilver were capable of producing them. It is worthy of observation also, that the *same* pressure which produced an *increment* in the rate of the former chronometer, occasioned a *decrement* in that of the latter; and by a reference to several of the foregoing experiments, it will be found that the time-keeper D in all cases *gained* with *less* pressure, and the chronometer F the contrary.

Changes produced in the rates of the Chronometers D, F, in consequence of regular decrements in the pressure denoted by an inch of mercury.

Mean Temp.	Mean Pressure.	No. of Days.	Mean daily rate of D.	Mean Temp.	Mean Pressure.	No. of Days.	Mean daily rate of F.
° 51	30.4	5	″ + 4.2	° 51	30.4	5	″ + 3.9
50	29.4	4	+ 6.4	50	29.4	4	+ 3.1
50	28.4	4	+ 7.4	50	28.4	4	+ 2.5
48	27.4	4	+ 8.0	48	27.4	4	+ 2.1

In the next set of experiments, the time-keepers A, C, F, and R were employed. The results are recorded in the next table; and it will be perceived that a difference of pressure amounting only to 0.69 inches of mercury, for twenty-four hours, was capable of producing an alteration in the rate of F, of the same kind as observed in the experiments with

the same chronometer for Geneva, Madrid, Chimborazo, &c. The time-keeper R likewise lost with a diminution of pressure. The chronometers A and C received increments to their rates, in perfect conformity to those obtained in former experiments.

Changes produced in the rates of the chronometers A, C, F, R, in consequence of small but irregular decrements in the pressure.

Mean Temp.	Mean Pressure.	No. of days.	Mean rate of A.	Mean rate of C.	Mean rate of F.	Mean rate of R.	Diff. of pressure.	Change of rate in A.	Change of rate in C.	Change of rate in F.	Change of rate in R.
°	Inches.		″	″	″	″	Inches.	″	″	″	″
47	29.26	4	−2.9	+3.4	+2.5	−6.5					
							−0.96	+1.4	+0.5	−1.2	−0.8
49	28.30	4	−1.5	+3.9	+1.3	−7.3					
							+1.38	−3.2	−1.8	+0.3	+0.9
50	29.68	4	−4.7	+2.1	+1.6	−6.4					
							−1.68	+0.6	+0.8	−1.2	−1.1
48	28.00	4	−4.1	+2.9	+0.4	−7.5					
							+1.69	—	—	+0.8	—
46	29.69	4	—	—	+1.2	—					
							−0.69	—	—	−1.7	—
42	29.00	4	—	—	−0.5	—					

To throw as much light as possible on this very interesting and important part of the investigation, six distinct sets of experiments were performed with the pocket chronometer H, and which, as before remarked, has been found uniformly to maintain an excellent rate when under a constant atmospheric pressure; and to display an immediate alteration whenever the density of the air was sensibly changed. In the succeeding table, it will be observed by inspecting the fifth and sixth columns, that in every case the rate of the time-keeper was *increased* as the density was *diminished*, and *vice versa;* and also, that an alteration of 0.6 inches in the mercurial column produced a sensible alteration in the time-keeper. The want of perfect proportionality in the rates,

may be attributed to the minute aberrations of the chronometer, produced by imperfections of workmanship, and to the errors arising from observation.

Changes produced in the rate of the time-keeper H, by small but irregular decrements in the pressure.

Mean Temp.	Mean Pressure.	Number of Days.	Mean daily rate.	Difference of Pressure.	Change of rate.
°	inches.		″	Inches.	″
51	29.26	4	+ 8.7		
				— 0.96	+ 1.0
49	28.30	4	+ 9.7		
				+ 1.38	— 0.4
47	29. 8	4	+ 9.3		
				— 1.68	+ 1.4
46	28.00	4	+ 10.7		
				+ 1.69	— 1.3
45	29.69	4	+ 9.4		
				— 1.69	+ 1.2
48	28.00	4	+ 10.6		
				+ 2.25	+ 1.2
47	30.25	4	+ 9.4		
				— 1.25	+ 0.4
44	29.00	4	+ 9.8		
				+ 1.02	— 0.3
43	30.00	4	+ 9.5		
				— 0.77	+ 0.5
42	29.25	4	+ 10.0		
				+ 0.60	— 0.4
40	29.85	4	+ 9.6		
				— 0.66	+ 0.6
40	29.25	4	+ 10.2		

From these experiments it may be therefore inferred, that a difference in the density of the atmosphere, represented by a quantity less than an inch of quicksilver, if continued for a day, was capable of affecting all the chronometers employed; and this is an atmospheric change by no means uncommon in this variable climate. Nor is it indeed neces-

sary that the alteration of density should even continue for twenty-four hours, since, from the change of rate being instantaneous (as will be proved in a subsequent page) six hours will be sufficient, in some cases, to disclose it. In cases however where the variations of the mercurial column are but small, and its transition from one state to another marked by a gradual character, the effect on the generality of chronometers is scarcely, if at all perceptible.

With a difference in the mercurial column of an inch and three quarters, or two inches, I have little doubt but all time-keepers will be influenced; and it is moreover known, that from a species of reaction in the atmospherical columns, it not unfrequently happens that the greatest depression of the barometer succeeds to a considerable elevation of it, and *vice versa*, so as to exhibit a difference of this kind. In the instance of the remarkable depression of the barometer, in December 1821, Mr. HOWARD informs us it sunk on the 25th instant to 27.83 inches, and on the 27th remained for 12 hours stationary at 28.07 inches; and from which time to the 31st it rose to 30 inches. Now, many examples might be selected from the experiments recorded in the preceding pages, to prove that a difference of two inches in the barometer, for twelve hours, would be sufficient to produce an alteration of rate; and there can be little doubt, that had the rates of some good chronometers been carefully attended to* during this singular alteration of atmospheric density, variations of rate, at least equivalent to that produced by

* I have attempted, but without success, to obtain the rates of some good chronometers during this period.

transporting a time-keeper from London to Geneva, would have been observed.

The sudden changes to which the density of the atmosphere is sometimes liable in this climate, renders it necessary, therefore, that a correction should be applied to the rate of a chronometer, proportional to the alteration of density; the correction partaking in some cases of a positive character, and in others of a negative. A similar correction must likewise be necessary when a traveller ascends to any considerable elevation above the sea; for example, to Geneva, to the plains of the Castiles, or to the table land of Mexico. The value of the correction will be different for different time-keepers, and in all cases must be determined by previous experiment.

The changes here alluded to can influence chronometers only beyond the tropics, since between them, it is known that the fluctuations of the barometer do not much exceed a quarter of an inch; but in the arctic regions, where the causes which promote alterations of atmospheric density are the greatest, the effect on the time-keeper must be the greatest also.

In proportion however as we ascend above the level of the sea, the uncertain changes of the barometer are known to approximate to uniformity; and therefore at higher elevations, the same chronometer would preserve a greater regularity of rate than in the lower regions of the air.

It becomes now an interesting question to determine, if the alterations of rate displayed by the different chronometers, under the various circumstances in which they have been placed in the preceding experiments, is *immediately* acquired the

moment the change of pressure takes place; or whether it is an effect which the air gradually produces on the machine.

By a reference to the experiments performed with the box chronometer E, it would seem as if the alteration of pressure required two days to produce its full effect on the rate; but from other experiments now about to be recorded, and on which I place a greater dependence, it would appear that the change is *immediate.*

A pocket and box chronometer, possessing detached rates of + 2″.0, and + 1″.9, were placed under the receiver in air denoted in density by 2 inches of the mercurial column; and which great degree of exhaustion was employed in order that, by producing considerable alterations of rate, the changes during very small intervals of time might be perceptible.

At the expiration of an hour, the increment produced in the rate of the pocket chronometer by a mean of three observations was found to be + 1″.33; whereas the detached rate in the same time would have amounted only to + 0″.37, being a clear increase of 0″.96 in consequence of the diminished pressure. At the end of the second hour the mean rate was found to be + 1″.23; and in like manner at the termination of the third + 1″.35; of the fourth + 1″30; of the fifth + 1″.10; of the sixth + 0″.80, and at which rate it continued for several hours. At the end of the nineteenth hour the rate recovered itself and became + 1″.28; at the twenty-second hour + 1″.25, the twenty-third + 0″.80, and at the twenty-fourth + 1″.05. These different results may, in a practical point of view, be regarded as uniform, considering the unavoidable errors of observation in attempting to

estimate the exact value of such minute inequalities. Indeed, the mean of the hourly observations from noon to midnight presented the same result as the mean from midnight to noon, the former being + 1″.12, and the latter + 1″.10. The entire rate for the twenty-four hours amounted to + 26″.6, being an increase on its detached rate of 17″.6. In like manner, the mean of three comparisons for the first of the horary observations with the box chronometer presented a rate of + 6″.76; whereas the detached rate during the same time would have been + 0″.08, exhibiting an increment of 0″.68 due to the diminished density of the air. By continuing the horary observations during the twenty-four hours, it was found that the mean of the horary rates for the first twelve hours was + 0″.92, and of the last + 0″.72. The entire rate for the whole period was + 20″.4, being an increment to its detached rate of 18″.5.

The succeeding day the two chronometers were restored to the full pressure of the atmosphere; and the first hour after their restoration, an attempt was made to discover the same increment in the rate of the pocket chronometer as that which it possessed under the receiver, but without effect; the mean of four comparisons giving only a rate of + 0″.45, an increment bearing evidently a relation to the primitive detached rate of the time-keeper. At the end of three hours the mean rate per hour was found to be + 0″.40; at the end of six hours + 0″.46; at the expiration of nine hours + 0″.41; at the end of eighteen hours + 0″.39; and at the end of twenty-four hours + 0″.44; the mean of the whole being + 0″.42, and producing a rate for the entire twenty-four hours of + 10″.08. Similar observations with the box

chronometers produce a rate agreeing exceedingly near with mean time; the entire rate for twenty-four hours being $+1''.0$, agreeing very nearly with its former detached rate.

Hence it may be inferred, that the change produced in the rate of a chronometer by *diminishing* the density of the air, is *immediate* and *uniform* in its effects; and so also is the effect produced by *increasing* it.

It may not be uninteresting to furnish a few examples illustrative of the power which most chronometers have of regaining their original rates, or very nearly so, after they have been subjected to pressures, both considerably above and below the mean density of the air; a property by which they are enabled to recover any temporary derangement they may undergo. The following instances are selected from the time keepers placed under the receiver of the air pump. The detached rates are those obtained under the ordinary circumstances of the atmosphere. The pressure to which the chronometers were subjected, are denoted by the several inches of quicksilver.

Examples of Chronometers having nearly regained their original rates, after being subjected to different Pressures under the Receiver.

	″		″		″		″
Detached	+ 4.9	Detached	− 6.3	Detached	+ 7.5	Detached	+ 4.4
3 in.	− 23.8	5 in.	+ 4.3	5 in.	+ 26.9	5 in.	+ 21.2
Detached	+ 2.5	Detached	− 5.9	Detached	+ 8.7	Detached	+ 3.4
Detached	+ 7.9	Detached	− 1.9	Detached	− 5.9	Detached	+ 6.5
12 in.	+ 19.2	15 in.	+ 4.2	20 in.	− 4.0	26 in.	+ 9.5
Detached	+ 8.2	Detached	− 2.6	Detached	− 6.4	Detached	+ 7.3

The following are examples derived from the experiments performed with the condenser.

Examples of Chronometers having nearly regained their original rates, after being subjected to different Pressures in the Condensing Engine.				
Detached + 7″.3 40 in. + 3.1 Detached + 7.4	Detached + 4″.3 75 in. − 17.2 Detached + 4.5	Detached + 0″.2 75 in. − 9.5 Detached − 0.6	Detached + 7″.5 90 in. − 2.2 Detached + 8.7	Detached + 4″.4 90 in. − 5.0 Detached + 3.4

A similar restoration of rate was found also to take place by removing a chronometer from rarefied to condensed air, and afterwards restoring it to air of the previous density, as exemplified in two instances in the next table.

Examples of Chronometers having nearly regained their original rates, after being removed from rarefied to condensed air.			
Mean Pressure.	Mean daily rate.	Mean Pressure.	Mean daily rate.
23 inches. 90 inches. 23 inches.	+ 11″.6 + 1 .6 + 12 .8	23 inches. 90 inches. 23 inches.	+ 5″.5 − 4 .4 + 6 .1

It was also found, that by restoring the above chronometers to air of any other density than that from which they were originally taken, the rate would not return to its primitive quantity. When, for example, they were placed under a pressure of 21 inches, the rate of the first chronometer became + 14″.2 instead of + 11″.6; and the daily increase of the second + 7″.2, instead of + 5″.5; results in exact conformity with the general law which regulated the same chronometers in other experiments.

As a striking example of the power possessed by a chronometer, of immediately altering its rate to a quantity corresponding with every new circumstance in which it was placed, and also of regaining its original rate, after it was again restored to its primitive condition, the following table may be referred to; and in which it will be perceived, that the original rate with which the chronometer departed, was still possessed within the small quantity 0.″2, after the last experiments. During the course of observations, the time-keeper was subjected to pressures from 60 inches to 3 inches; and it will also be perceived, that the greatest deviation of the detached rate from the original rate, amounted only to 0″.8. The observations embraced a period of four months, and during which the temperature varied from 39° to 60°.

Detached	+ ″7.9	Detached	+ ″7.2
28.6 in.	+ 9.7	34 in.	+ 5.3
Detached	+ 8.0	Detached	+ 7.0
21 in.	+ 14.2	38 in.	+ 2.3
Detached	+ 7.9	Detached	+ 8.0
15 in.	+ 17.0	5 in.	+ 26.6
Detached	+ 8.2	Detached	+ 7.5
12 in.	+ 19.0	5 in.	+ 26.9
Detached	+ 7.8	Detached	+ 7.9
60 in.	— 6.6	40 in.	— 3.0
Detached	+ 8.7	Detached	+ 7.7
3 in.	+ 28.0		

The occasional imperfections of the valves of the condensing machine and of the air pump, have likewise afforded some interesting proofs of the truth of this investigation.

In more than one instance it has happened, that the mercurial column of the gauge of the air pump has been elevated during the intervals of comparison, by the introduction of air; and the effect of which was in all cases, an alteration of rate in the time-keeper, dependent on the degree of change. For example, during the period when a pocket chronometer was subjected to air of a density represented by 12 inches of quicksilver, and when its rate was + 10″.0, the gauge became imperfect, the quicksilver rising to 18 inches soon after the time of comparison; the succeeding day the rate was + 7″.5. Another chronometer under the same circumstances also underwent an alteration in its rate from + 18″.0 to + 14″.2. In another instance, the rate of the time keeper was + 15″.0 under a pressure of 4 inches; during the next twenty-four hours the quicksilver rose to 12 inches, and the rate declined to + 12″.4. The next day the imperfection of the gauge still continuing, the mercury rose to 17½ inches, and the rate sunk to + 9″.9.

Sometimes a part of the air contained in the condensing engine, by escaping, would occasion fluctuations in the rates of the time keepers; but on restoring the machine to perfect order, they would immediately assume an uniform character. Thus, when a box chronometer was placed in air of uniform density, corresponding to 45 inches of mercury, the rates for three days were respectively — 10″.9, — 10″.6, and — 10″.5; but on attempting to increase the density to 48 inches of quicksilver, the machine disclosed proofs of imperfection; and the rates for three days were respectively —15″.0, —12″.8, and —10″.1. The machine was then repaired, and air introduced into it of a density denoted by 51 inches

of the mercurial column, when the daily rates were — 16″.0, — 15″.7, and — 16″.0. In another instance, a time-keeper maintained a steady rate of — 10″.5 under a pressure of 40 inches. During one day some of the air escaped, and the rate became — 7″.0. The machine was then repaired and the former pressure restored, when the rate returned to — 10″.3. On the third day air again escaped, and the rate changed to — 8″,5; but on the fourth day the condenser being in perfect order, the time-keeper again returned to — 10″.7. Thus, even from the temporary imperfections of the air pump and condensing machine, may proofs be drawn, demonstrating the effects of atmospheric pressure on the rates of chronometers.

An experiment was also attempted, at the suggestion of Mr. DAVIES GILBERT, to discover if, by removal of the case of a chronometer, and presenting its balance and spring to the free action of the air, the effect on the rate would present a result analogous to that which it would furnish under ordinary circumstances. With the case of the time-keeper in its proper situation, and the full pressure of the atmosphere on the machine, the rate of six days was — 15″.3; but under the receiver with the mercurial gauge at 26 inches, it became for a like period — 13″.6. By removing the case, the detached rate became — 16″4; and under the receiver, with the pressure of 26 inches, the rate was — 15″.8; the removal of the case having in both instances increased the losing rate of the machine. On placing the time-keeper in the condensing machine under a pressure of an atmosphere and a quarter, the case being still removed, the rate was found to be — 15″.5, differing but little from that determined in its

detached state. By augmenting the quantity of air to an atmosphere and a half, the daily variation still presented the same quantity; but on removing the chronometer into the receiver under a pressure of 13 inches, the rate became $-13''.3$; and with a pressure of 12 inches, $-8''.8$. Hence it appears, though the experiment is too limited to draw from it any general conclusion, that in this particular case, no alteration of rate took place by increasing the density of the air above its average state; but by diminishing it *below* the same point, the rate was accelerated.

It may be proper however to advert to a circumstance, which may be regarded as having possibly exercised some influence on the foregoing results; and that is, the change of temperature always attendant on sudden alteration in the density of the air.

When the chronometers were placed, for example, beneath the receiver of the air pump, a sudden diminution of temperature was in all cases produced, proportional to the degree of exhaustion; and on restoring the medium to its original density, an elevation of temperature resulted, equally rapid in its effects. So also when the time-keepers were placed in the condensing engine, the sudden compression of the atmosphere, produced an immediate liberation of caloric, and which was followed, when the equilibrium of air was renewed, by a depression of temperature below its original condition.

These alterations of temperature, although producing a change of only two or three degrees, in a delicate thermometer, are nevertheless regarded by Mr. Dalton as the effect of a much greater degree of heat; but which is permitted to impart its influence only for a very few seconds, in

consequence of the immediate effort made by the receiver, and the surrounding objects, to restore the primitive temperature. Now, although a sudden change of temperature, of forty or fifty degrees, if allowed to maintain its influence for any considerable time, would perhaps in some cases produce a small derangement of rate; yet, from its continuing to act only for a few seconds, and producing, even on a sensible thermometer only a very small effect, it cannot be supposed that the adjustments for temperature in the balance of a chronometer, protected as they are by the thick case of the machine, can be in the least degree influenced by it. Accordingly, on introducing a very susceptible time-keeper, H, into an atmosphere 50° warmer than the ordinary state of the air, for ten seconds, no visible alteration of rate resulted; and it may hence may be inferred, that the changes of temperature which have taken place during the preceding experiments, in consequence of variations in the density of the air, can have had no share in producing those changes of rate which have been perceived; and that they resulted from alterations of pressure alone.

A change of rate in a chronometer, from an alteration in the density of the medium in which it is placed, considered as a simple fact, seems demonstrated from the foregoing experiments. Different hypotheses may probably be advanced respecting the cause; but, the supposition which appears the most probable is, that a change takes place in the arc of vibration of the balance, in consequence of the altered density of the air, and a consequent variation in the rate of the timekeeper, from the imperfect isochronism of the balance.

The true measure of time is derived from the balance, and its vibrations will be isochronous, when the adjustments of the spiral spring are such as to admit of its elastic force varying directly with the arcs of vibration. As a necessary consequenee of this principle, the application of any disturbing force will occasion no derangement in the rate, since the arcs of vibration, whether increased or diminished, must all be performed in the same absolute time.

It may however be questioned, whether a chronometer ever existed, in which the elastic force of the spring varied precisely with the arcs of vibration; for it has been shown by Mr. Atwood,* that though the weights employed to counterpoise the elastic force of a spring, at different angular distances from its quiescent position, may *appear* to be in the *exact* ratio of those distances, discrepancies too small to be detected by the nicest experimenter may exist, but yet be considerable enough, from the delicate nature of the machine, to create a sensible alteration of rate.

Any change in the arc of vibration of the balance of a chronometer thus constituted, must therefore be attended with some alteration of rate; and that it is extremely probable such a change must take place when a time-keeper is placed in air of different densities, the arcs *increasing* when the density is *diminished*, and *diminishing* when it is *increased*, may be inferred, from the extreme delicacy of the balance, and its spring, and their consequent susceptibility of change.

The daily aberration of a chronometer, arising from a

* Philosophical Transactions for 1794.

change in the arc of vibration, has been demonstrated by Mr. ATWOOD to be the following function.

$$24^{h}\left\{\left(\frac{a}{a'}\right)^{\frac{1-n}{2}}-1\right\}$$

in which a denotes the primitive arc of vibration, a' the arc resulting from the action of a disturbing force, and which, in addition to the case assumed by the abovementioned philosopher, may be regarded as either greater than a, or less than it; and n, the exponent, denoting the ratio between the elastic force of the spring, and the angular distances from the point of quiescence.

Supposing the primitive arc constant, the above function will undergo different modifications, according to the values assigned to the elements a' and n.

In the first place, we may conceive the elastic force of the spring to vary directly with the angular distances from the point of quiescence, the exponent n being, in this case, denoted by unity. This supposition, by causing the whole function to vanish, will indicate a perfect isochronism; and that therefore, whatever magnitude be attributed to the arc a', in consequence of the action either of rarefied or condensed air, no alteration of rate will result. And it is remarkable that, during the whole course of experiments, I have not met with an instance to illustrate this case. Every chronometer examined exhibited a tendency either to gain or lose, when the density of the air was changed; and which therefore proves, that perfect isochronism is seldom if ever attained in the construction of a time-keeper.

Secondly, we may assign to the exponent n, a value *less*

than unity; or, which is the same thing, we may suppose the elastic force of the spring to vary in a *less* ratio than that of the angular distances from the point of quiescence; and which hypothesis will produce different results, according to the values assigned to a'; for if the time-keeper be placed in condensed air, so as to make a' less than a, a *positive* value will be given to the function, or the chronometer will *gain*. If, on the contrary, the time-keeper be placed in rarefied air, so as to make a' greater than a, still preserving the magnitude of n, the numerical value of the formula will be *negative*, and the chronometer *lose*. Cases to illustrate both suppositions are recorded among the experiments.

Thirdly, we may suppose the elastic force of the spring to vary in a greater ratio than the angular distances from the point of quiescence, and in which case n must be greater than unity. If then we suppose the chronometer to be placed in condensed air, the value of a' becoming in such a case *less* than a, the numerical value of the function will be *negative*, and a retardation of rate will be the necessary result. But if the time-keeper be placed in rarefied air, so as to make a' *greater* than a, preserving the same value of n, the numerical result of the formula will be *positive*, and the time-keeper must *gain*. And this, according to the foregoing experiments, is by far the most general condition of chronometers. It may also be inferred from the same circumstance, that time-keepers are more frequently constructed with the elastic forces of their springs, varying in a *greater* ratio than the angular distances from the point of quiescence, than the contrary.

The preceding suppositions will therefore explain why

some chronometers, during the preceding experiments, *gained* with *diminished* pressure, and *lost* with *increased;* whilst others possessed properties precisely the reverse.

In determining the rates of the time-keepers, I had the advantage of a transit instrument, and an astronomical clock.

Plymouth, March 1, 1824.

XXI. *A Letter from* Lewis Weston Dillwyn, *Esq. addressed to Sir* Humphry Davy, *Bart. P. R. S.*

Read March 25, 1824.

My Dear Sir,

Through you I beg leave to offer to the Royal Society, some further observations on the relative periods at which different families of testaceous animals appear to have been created, and on the gradual approximation which may be observed in our British strata, from the fossil remains of the oldest formations to the living inhabitants of our land and waters.

The series of strata beginning with transition lime and ending with lias, contains shells belonging to various genera of conchifera, cephalopoda, annelides and herbivorous trachelipoda; and also some other shells, as for instance, the multilocular and spiriferous bivalves, which cannot be referred to either of those natural orders, or groups of genera, into which all the other testacea, both recent and fossil, have been divided. In the simple bivalves belonging to these strata, the marks which best serve to distinguish their families are generally obliterated, and but little more can with any certainty be observed, than that the two orders into which Lamark has divided them, have existed together throughout every formation from transition rocks to the present day. An examination of the few perfect specimens which I have met with, however, leads me to suspect that all the dimyairia

of these strata have the ligament external, and consequently, that internal ligaments were confined to the monomyairia, till after the lias had been deposited.

In the secondary beds above the lias, all the shells may be referred to some of our now existing orders of animals, and the extinction of the unknown orders is immediately followed by the first appearance of another order of mollusca, to which LAMARCK has limited the name of gasteropoda, and, as was first suggested to me by Mr. MILLER, all those fossils of the older strata, which have been supposed to be inside and outside casts of patellæ, were obviously formed in the concave sides of the vertebra, or by the intervertebral cartilages of a fish. As a few of the carnivorous trachelipoda are said to have been found in the oolites, their first appearance may probably be referred to the same epoch; but I have not myself been able to detect either of the families of this section of trachelipoda in any secondary bed, excepting the denuded tracts of green sand in Devonshire; and there, perforations exactly similar to those which abound among tertiary and recent shells are also of frequent occurrence, although I have never met with any such perforation in any other secondary formation, nor even in any of those regular beds of green sand, which actually underlie the chalk in other counties. I am not enough of a geologist to decide, as to whether any admixture of secondary and tertiary fossils may possibly have taken place when these denudations were made, but I can in no other way account for the fact, that all the species which have been perforated, as well as the carnivorous trachelipodes themselves, are nearly similar to those of the London clay; and I have never been able to find any

such perforation in either of those species which are found in the more regular beds of green sand, and which are sometimes mixed with them. These perforations may be readily distinguished from those more oblique and lateral burrowings which are often found in secondary fossils, and are always conveniently formed for suction by being broadest in the outer surface, and go straight through that part of the shell which is immediately over the animal; whereas in the latter the holes are cylindrical, and much more resemble the indiscriminate burrowings which are common in recent oyster shells.

In my former Letter, which the Royal Society has done me the honour to publish in the Philosophical Transactions of last year, I have pointed out some of the changes which took place immediately after the chalk formation was completed; and of the British strata it may be further observed, that it is only in the tertiary beds that any traces of the cirrhipeda, or of any of the families of naked mollusca have been found. The beak, which has been figured by BLUMENBACH, and which among the fossils of the lias is mentioned by CONYBEARE and PHILLIPS as the beak of a sepia, belonged, as I think, unquestionably, to the cephalopode animal of an ammonite; and it sufficiently resembles the lower mandible of the parrot-like beak which RUMPHIUS has described of nautilus pompilius. As might be expected, if these mandibles, or rather casts of mandibles, belong to the ammonites, they differ generically in shape from those of every living genus of chephalopoda which has been figured or described, and I have found them in all those beds; and, so far as I can

ascertain, they have been discovered in those beds only of the lias, lower oolite, and chalk, which contain the larger ammonites. From the greater tenuity of these beaks in the smaller species, they may probably have yielded to pressure, and decay before the mud which filled them had become sufficiently hard to retain their shapes; and as the lower mandibles of the cephalopoda are always much larger and thicker then the upper ones, the non-appearance of any of the latter may be accounted for in the same way. The sepiæ are moreover furnished with one of those thick dorsal plates which are commonly called cuttle-fish bones, and most, if not all the other sepiadæ, contain an internal horny substance of the same nature, which is generally at least as thick and durable as the mandibles; and if the fossil beaks of the secondary strata belonged to this family, then, in all probability, some of the dorsal plates would be found with them; but nothing of the kind has been discovered in any older British stratum than the London clay. So far from being able to detect any traces of the naked mollusca, I have not been able to find, in the secondary strata, either of those shells by which the animal is only partially covered, nor any of those of the convolutæ, which necessarily change their shells at different periods of their growth, and of which the animal must therefore occasionally remain exposed, till a fresh coat of calcareous matter has been secreted. In my former Letter I have stated, that all the marine spirivalves of the secondary strata belong to operculated genera, and these observations serve still more strikingly to prove that, till the chalk deposits were completed, the mollusca, in our

latitude, required a more perfect protection either from their enemies, or from the surrounding element, than afterwards became necessary.

The same gradual approximation towards recent shells, which may be traced in the older strata, is also carried on through the tertiary formations, and the affinity, which is complete with respect to orders in secondary beds above the lias, becomes further extended, and every tertiary shell may be referred to some existing genus; but though the approximation has proceeded thus far in the London clay, yet all its immensely numerous species are now extinct; and it is only in those uppermost beds of crag, which lie between the London clay and our present creation, that any fossil can be completely identified with a living species: the shells which may be thus identified are however mixed with many extinct species; and though the fossils of the crag appear generally to have belonged to a warmer climate than ours, yet their character is much less tropical than those of the London clay, and in every respect they all approach nearer to the present inhabitants of the British coasts.

I have already observed, that the shells of unknown families are confined to the beds below the lower oolite; and in all the upper formations a relationship is completed between fossil and recent shells in the following regularly approximating series. In the secondary strata above the lias as to *natural orders*, in the London clay as to *genera*, and partially as to *species* in the crag.

These observations refer exclusively to the animals of the 9th, 10th, 11th, 12th, and 13th classes of invertebrata in LAMARCK's arrangement; and whether the same sort of

regularly approximating affinity can be observed in the other classes, I must leave it for those who are more conversant with them to decide.

I remain, my dear Sir,
very faithfully your's,
L. W. DILLWYN.

Penllergare, Feb. 1, 1824.

Sir HUMPHRY DAVY, Bart. P. R. S. &c. &c.

XXII. *An account of the organs of generation of the Mexican Proteus, called by the natives Axolotl.* *By Sir* EVERARD HOME, *Bart. V. P. R. S.*

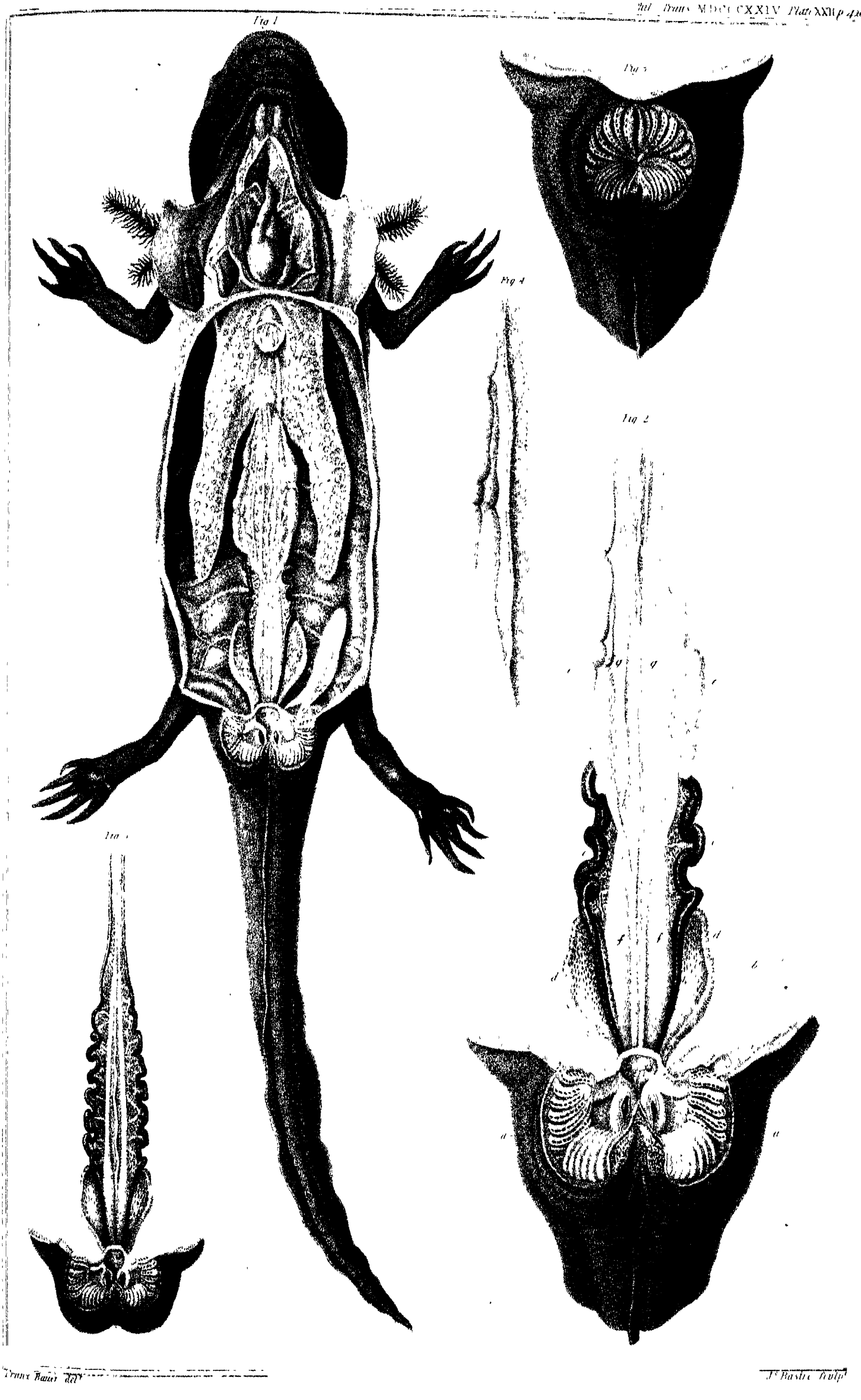
Read June 17, 1824.

THAT the Proteus from Germany, as well as that from Carolina, is an animal in its perfect state, I consider to have been proved by Baron CUVIER, in his account of the skeletons of these animals; and when I found that their vertebræ were cupped, which is not the case in the Aquatic Salamander, to which in many respects they are nearly allied, that circumstance alone, with me, distinguished them from all the lizard tribe.

Having had the opportunity of examining the vertebræ of the Proteus from Mexico, and finding them also cupped, I could have no doubt of its belonging to the same tribe, and consequently an animal in it's perfect state. This, however, required proofs, that could only be afforded by an examination of the organs of generation in a developed state.

When Mr. BULLOCK went to Mexico, I requested him to bring me some specimens of this animal, and to collect information respecting it's habits, more particularly its mode of generation.

In compliance with my request, Mr. BULLOCK brought me several specimens: they were found in a lake three miles from the city of Mexico. The temperature of the water is never below 60°, and the elevation of the lake above the



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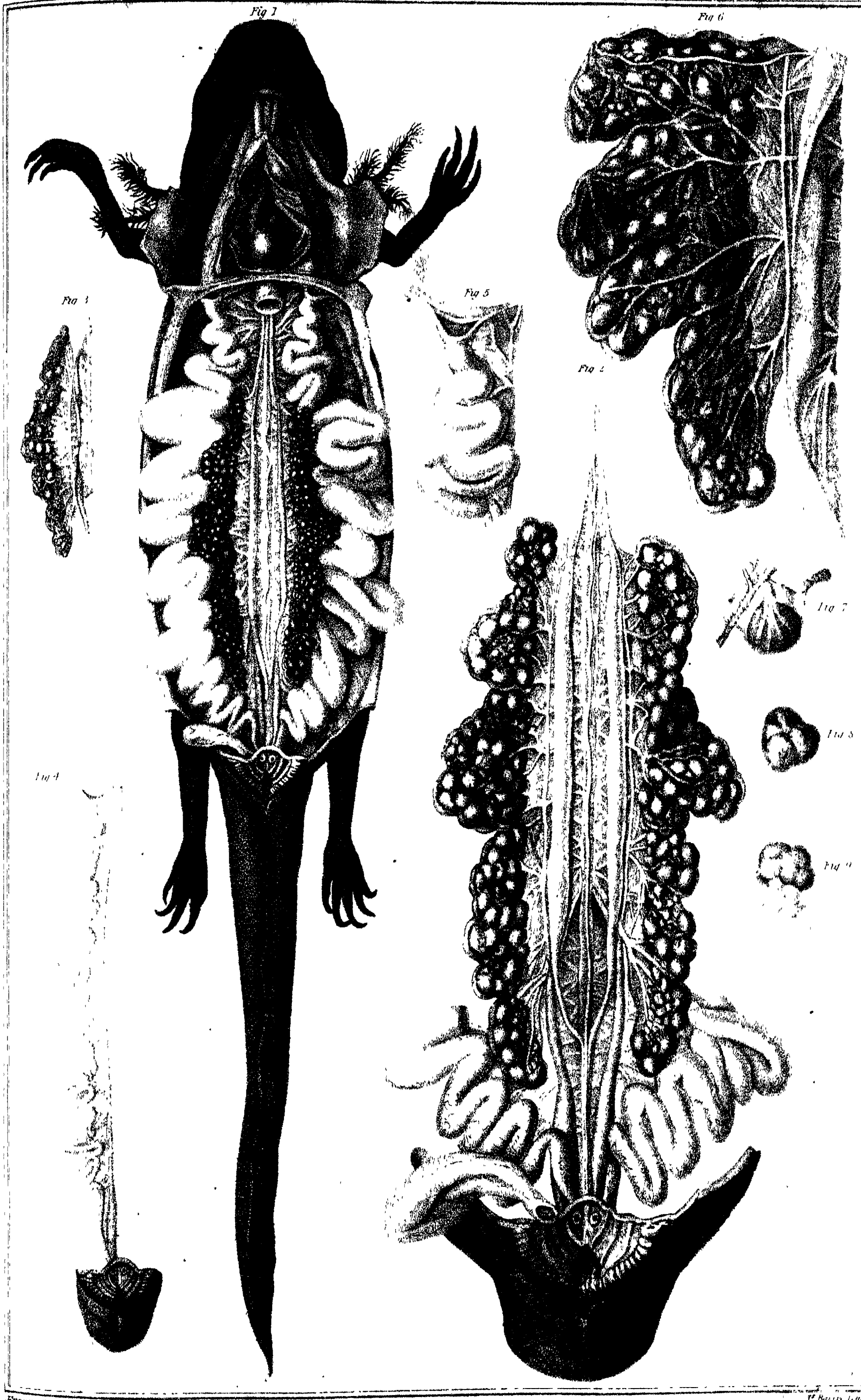

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place between the external parts of the male and female, those of the male appear to surround and enclose those of the female, contrary to what happens in other animals.

The female organs, in their developed state, are beautifully shown in the annexed drawings; and from the appearance of the ova, they probably pass out singly.

Now that the three different kinds of Proteus are ascertained to be perfect animals, Mr. RUSCONI's attack upon Mr. HUNTER's want of sagacity, published at Milan in 1821, in his work, entitled "Amours des Salamandres Aquatiques," will revert upon himself and his friend Mons. de LACEPEDE, who too hastily concluded them to be larvæ.

EXPLANATION OF THE PLATES.

PLATE XXI.

Shows the external appearance of the organs of generation.

Fig. 1. The male organs.

Fig. 2. The female organs; both of the natural size.

PLATE XXII.

Shows the male organs in the different stages of developement.

Fig. 1. The male Proteus laid open to show the heart, lungs, and gills in situ, as well as the organs of generation. For an explanation of these, take the letters of reference annexed to Fig. 2.

Fig. 1, is of the natural size.

Fig. 2. The organs of generation removed from the body; magnified two diameters.

a a. The external orifice composed of plicæ.

b. The urinary bladder.

cc. What corresponds to vesiculæ seminales.

dd. What corresponds to COWPER'S glands, being met with in both sexes.

ee. The testicles.

ff. Kidneys.

gg. Fatty bodies similar to those in the frog.

Fig. 3. The external orifice expanded; magnified two diameters.

Fig. 4. The testicle and the longitudinal mass of fat annexed to it; magnified two diameters.

Fig. 5. The testicles having been removed, what corresponds to the vesiculæ seminales and COWPER'S glands are exposed, and the kidneys lying between them; the parts are of the natural size.

PLATE XXIII.

Shows the female organs in the different stages of developement.

Fig. 1. The female Proteus laid open to show the ovaria and oviducts in the state of complete developement just before the ova are shed. The parts of the natural size. The orifices of the oviducts to receive the ova are expanded for that purpose.

Fig. 2. The ovaria and oviducts removed from the body, and magnified two diameters. The urinary bladder is distinctly seen, as well as the masses of fat lying between the ovaria; the parts are magnified two diameters.

Fig. 3. The ovarium of a Proteus in a virgin state. The parts of the natural size.

Fig. 4. The oviduct in the virgin state; of the natural size.

Fig. 5. The funnel-like opening of the oviduct when prepared to receive the ova; magnified four diameters.

Fig. 6. The ova inclosed in the ovarium just before they are shed, and the mass of fat that lies close to the roots of their blood vessels; magnified four diameters.

Fig. 7. An ovum with its natural covering, removed from the ovarium; magnified six diameters.

Fig. 8. An ovum laid bare; magnified six diameters.

Fig. 9. An ovum laid open; magnified six diameters.

XXIII. *An account of Experiments on the velocity of Sound, made in Holland. By Dr.* G. Moll, *Professor of Natural Philosophy in the University of Utrecht, and Dr.* A. Van Beek.

Read March 18, 1824.

Sir Isaac Newton's formula, expressing the velocity of sound,

$$c = \sqrt{\frac{gp}{D}}$$

has since his time been investigated and demonstrated by several first-rate mathematicians. Actual experiments however on this velocity, instituted in various countries, and under different circumstances, went to prove that the celerity of sound, found by experimiment, is about one-sixth greater than can be deduced by theory.

The celebrated Laplace accounted for this difference between experiment and theory, by showing that it could be attributed to the heat evolved by the compression of the particles of air which is effected by the undulations of sound. It was found impossible to determine the quantity of heat thus evolved, by the compression which sound occasions in the particles of the air; and therefore it was deemed expedient to multiply Sir Isaac Newton's formula by a constant factor $\sqrt{1 + k}$, the value of which was determined by experiment. Sir Isaac's formula thus altered, became

$$c = \sqrt{\frac{pg}{D}} \cdot \sqrt{1 + k}.$$

Thus, by the experiments of the French Academicians of

1738, the most accurate on this subject of that time, the value of *k* was found equal to 0,4254. It is plain that this correction of the original formula is merely empirical, and dependant on the accuracy of experiments, which in 1738, had certainly not attained the perfection which is required at present.

In consequence, this formula was thus altered by LAPLACE,

$$\sqrt{\frac{\sqrt{g\,p}}{D}} \cdot \sqrt{\frac{c'}{c}};$$

in which c' is the specific heat of the air under a constant pressure, and c is the specific heat of the air under a constant volume.*

My friend Dr. VAN REES, Professor in the University of Liege, gave a demonstration of this correction $\sqrt{\frac{c'}{c}}$, which will be subjoined to the present paper,† and which may be compared with that of Mr. POISSON.‡ The value of $\frac{c'}{c}$ was determined by LAPLACE from experiments of Messrs. LAROCHE and BERARD,§ and found equal to 1,4954; but later and more accurate experiments of Messrs. GAY LUSSAC and WELTER brought it to 1,3748.

Another cause of the difference between actual experiments on the velocity of sound and its theory, exists in the variable force of the wind, which either accelerates or retards the velocity of sound, according to the direction from which it is blowing. It appears that this cause of error may be annihilated in the following manner. Let sounds be ex-

* LAPLACE in Ann. de Phys. et Chim. Tom. iii. p. 238.

† Dissertatio de celeritate soni, Trajet. 1818.

‡ Annales de Phys. et de Chim. Mai 1823, d. 5.

§ Ibid. Annales de Chimie, Tom. 85, p. 72.

cited exactly at the same time on both ends of a basis, and let two observers stationed on these ends, measure the velocity with which sounds travels from one end of the basis to the other. It is quite clear, that the action of the wind must necessarily accelerate the velocity of the sound excited at one end of the basis, as much as it will retard that at the other end, and thus the medium of these two velocities will give the velocity in tranquil air. This method was not adopted by the French Academicians of 1738, in their experiments between Monthlery and Montmartre. Cannon was fired at one of these stations, whilst the observers were at the other, and thus the results remained affected by the whole effect of the wind. It was found expedient therefore to repeat these experiments with more accuracy, and this was executed with great precaution on Mr. LAPLACE's proposal, by Messrs. ARAGO, PRONY, MATHIEU, BOUVARD, HUMBOLDT, and GAY LUSSAC. The experiments were made in 1822, on the basis of Monthlery and Villejuif. In two successive days, the 21st and 22d of June, 1822, seven shots were fired on both stations, and observed on the other; the difference of time in which the corresponding shots were fired at both stations not exceeding five minutes, and from these seven corresponding shots the result was deduced.

These experiments having never been made in this country with any thing like sufficient accuracy, His Royal Highness PRINCE FREDERICK, second son of HIS MAJESTY the KING of the NETHERLANDS, and Master General of the Ordnance, was pleased to approve of our proposal of repeating the same, and to authorise Lieutenant-Colonel KUYTENBROUWER, and the officers and men of the battalion of Artillery under

his command, to give us every assistance in their power, and to take an actual part in these experiments.

SECTION II.

As fitted places to make these experiments, two elevated spots were selected on the extended heaths of the Province of Utrecht. One of these is a small hill between the town of Naarden and the village of Blaricum, and called the *Kooltjesberg*; the other is somewhat higher, and situated on the right of the road from Utrecht to Amersfoort, and very near the last town. Both places were distinctly visible from one another, and the distance was between 17000 and 18000 metres (9664 fathoms). Our time was kept by two time-keepers, which the Minister of Marine had kindly furnished us with; one made by ARNOLD, the other by our countryman Mr. KNEBEL. But the exact interval between the observation of light, and the perception of sound, and consequently the velocity of sound, was measured with small clocks with conical pendulums. They are made at Wesel by Mr. WILHELM PFAFFIUS, and proved remarkably well adapted for this purpose. It is well known that HUIGENS laid down the properties of the conical, or centrifugal pendulum, but if we are not mistaken, they were employed for similar purposes for the first time by the German philosopher BENZENBERG.* These clocks with a conical pendulum divide the 24 hours of the day in 10,000,000 parts, and one of the indexes gives $\frac{1}{100}$ part of a decimal second. This index or second hand remains quiet, whilst the watch work continues moving as long as a certain spring is not pressed down with the finger; and on removing the

* Some account of these clocks is given in GILBERT's Annalen d. Physik, 1804. B. 16, p. 494; and New Series, vol. v. p. 333.

finger, the index is reduced to rest in the identical moment. Thus the index being at o, the spring is pressed down by the observer at the very instant the light of the opposite station is observed; the index continues moving till the report of the shot is heard, when the finger is withdrawn, and the index stopped instantaneously. The number of turns and fractions of a turn of the index shows the time elapsed between the fire and the report. There was a conical or centrifugal clock on each station; besides these, each station was furnished with a good barometer, carefully compared with a standard barometer of Mr. DOLLOND, several good thermometers made by Messrs. DOLLOND and NEWMAN, besides a sufficient number of excellent telescopes of DOLLOND'S, and so placed on stands adapted for the object as to bring the opposite station without trouble in the field of the telescope. The moisture of the air was determined for the first time in such experiments by Mr. DANIELL'S hygrometer, one of which was placed at each station. The direction of the wind was determined by very good vanes contrived by the Artillery officers. At each station a twelve pounder and a six pounder was planted, and the instruments were disposed in, or in the vicinity of tents erected for the purpose. Professor MOLL, with Lieutenants RENAULT and DILG, was stationed at the *Kooltjesberg*. Dr. VAN BEEK, with Lieutenants SOMMERTON, VAN DEN BYLAARDT and SEELIG were on the other station, which is commonly called *Zevenboompjes*, or seven trees, from the circumstance of seven trees being planted on this lonely elevation. Several gentlemen cadets of the Artillery, and several students of the University, were at both stations employed in observing the different instruments.

The barometers and thermometers were of course ob-

served in the open air; Mr. DANIELL's hygrometers were also placed in the open air; and the light of a candle reflected from the surface of the ball of the hygrometer, gave the means of observing the deposition of dew with great accuracy.

It was deemed of the utmost importance that the shots on both stations should be fired at as nearly the same moment as possible. To obtain this, the following plan was adopted. At $7^h\ 55'$ P. M. by the chronometer of Zevenboompjes, a rocket was fired at Zevenboompjes, which being observed at the other station of *Kooltjesberg*, was immediately answered by another rocket from the latter place. This was the signal that on both stations every thing was ready for observation. At $8^h\ 0'\ 0''$ by the chronometer of *Zevenboompjes*, a cannon shot was fired on that station, whilst the observers at Kooltjesberg took as exactly as possible the time on their chronometer when the light was observed. A second shot was fired at Zevenboompjes at $8^h\ 5'\ 0''$ P. M. by the chronometer of that station, and the time at which the light was seen was carefully taken down by the chronometer of Kooltjesberg. By these means the difference of the two chronometers at both stations, in a distance of about 9 miles, was ascertained with great accuracy; and in order to show that this preparatory investigation was made with due care, a cannon shot was fired on both places at the moment when the chronometer of Zevenboompjes marked $8^h\ 10'\ 0''$. If the lights of both shots were seen exactly at the same time, it was a proof that the difference of both time-keepers was known, and that experiments might be safely made.

We own that we did not suppose before hand, that it could

be possible to fire continually guns at a distance of 9 miles exactly at the same second; but the very great attention and ability of our artillery men overcame this difficulty. Between our shots at the two stations there was never a greater difference than 1″ or 2″, whilst this difference in the experiments of the French philosophers of 1822, went to 5 minutes. This exact correspondance in the firing of the guns was obtained in the following manner. At each station an officer had the chronometer placed before him on a small table very near the gun; a non-commissioned officer or gentleman cadet stood ready with the port fire near the touch-hole; and at the instant required the officer holding the chronometer pressed the arm of the person who was to fire the gun, which went off at the very moment. With a little practice they were certain to fire the gun at any given second.

The first night of our experiments, the 23d, 24th, and 25th of January, 1823, we experienced the same annoyance of which the French philosophers had to complain the first night of theirs. The report of the shots of Zevenboompjes was not heard at all at the station of Kooltjesberg. But at Zevenboompjes all the shots of Kooltjesberg were distinctly heard. After the first night we constantly used the metal twelve pounders loaded with 6lbs. of gunpowder. The 26th of January all the shots were heard at Kooltjesberg, but none were perceived at the opposite place. But the wind shifting the following night, a good number of corresponding or simultaneous shots were distinctly heard on both stations. The particulars of the experiments made in these different days will be found in the tables annexed to this paper. The dis-

appointment we met with on the first days was however not entirely fruitless; we were convinced by it, that none but exactly corresponding shots can be of use in determining the velocity of sound. The result of the observations of 25th and 26th of January, when the reports were heard at one station only, and reduced to 0° temperature of the centigrade scale, and dry air, give differences of $\frac{1}{53}$, whilst the observations of 27th and 28th of January, when shots were distinctly heard on both stations, had only a difference of $\frac{1}{502}$ from each other.

The time which sound employs to travel from one station to another being duly ascertained, we proceeded to measure the distance between both stations. The distances of the steeples of Utrecht and Amersfoort, Utrecht and Naarden, Naarden and Amersfoort being accurately known, we measured angles on our stations between these steeples, and on each steeple between the other steeples and the stations. Thus the distance was calculated by four different triangles, and the greatest difference between these calculations was $2^{m},45$ or 8 feet, which appeared of no consequence in these experiments. The distances of the different steeples which we took for our basis, result from the very exact geometrical survey of General KRAYENHOFF.*

From these different data we found, by calculations of which the detail will be given hereafter, that in our experiments at a temperature of 32° FAHRENHEIT, or 0° of the centigrade scale, the velocity of sound is $332^{m},049$, or 1089,7445 English feet per sexagesimal second. A table showing the

* Précis des Opérations Géodésiques et Trigonométriques en Hollande, par le Général KRAYENHOFF.

comparison of our experiments, with the observations of other philosophers, is also annexed to this paper.

SECTION III.

Observations to ascertain the distance between the stations of Kooltjesberg and Zevenboompjes.

A. Mensuration of angles with a ten-inch Repeating Circle, made by LENOIR, *in Paris.*

a. Angles measured on the steeple of the Cathedral of Utrecht.

α. Between Kooltjesberg and Amersfoort.

Date.	No.	Observations.	Repeated Angles.	Single Angles.	
			° ′ ″	° ′ ″	
1823. 1 Aug. 3h P.M.	1.	twice	98 22 30	49 11 15	Mean angle 49° 11′ 18″,2. Reduced to the center of station 49° 10′ 25″,2.
	2.	4	196 45	49 11 15	
	3.	6	295 9	49 11 20	
	4.	8	393 31 30	49 11 26	
	5.	10	491 52 30	49 11 15	

The reduction to the centre of station has been made by the well known formula $C = O + \frac{r \text{ Sin. } (O+y)}{D \text{ Sin. } 1''} - \frac{r \text{ Sin. } y}{G \text{ Sin. } 1''}$ taken from DELAMBRE's base du Systême Métrique, et PUISSANT's Traité de Géodésie.

β. Angles measured at Utrecht between Kooltjesberg and 7 Boompjes.

Date.	No.	Observations.	Repeated Angles.	Single Angles.	
			° ′ ″	° ′ ″	
1823. 1st. Aug. 3h P.M.	1.	Double	103 2 30	51 31 15	Mean angle 51° 31′ 9″,1. Reduced to center of station 51° 30′ 14″,1.
	2.	Quadruple	206 5	51 31 15	
	3.	Sextuple	309 6 30	51 31 5	
	4.	Octuple	412 9	51 31 7,5	
	5.	Decuple	515 10 30	51 31 3	

γ. *Angles measured at Utrecht between Amersfoort and the Pyramid of the Camp of Zeist.*

Date.	No.	Multiple Angles.	Repeated Angles.	Single Angles.	
			° ′ ″	° ′ ″	
1823. 1st. Aug. 3h P. M.	1.	Double	43 17 30	21 38 45	Mean angle 21° 38′ 39″; reduced to centre of station 21″ 38′ 0″,5.
	2.	Quadruple	86 35	21 38 45	
	3.	Sextuple	129 51	21 38 30	
	4.	Octuple	173 8	21 38 30	
	5.	Decuple	216 27 30	21 38 45	

b. Angles taken on tte pyramid of the Camp of Zeist between Utrecht and Amersfoort.

Date	No.	Multiples.	Repeated Angles.	Single Angles.	
			° ′ ″	° ′ ″	
1823. 7th Aug. 8h A. M.	1.	Double	225 50	112 40	Mean angle 112° 40′ 20″,1; angle taken from centre of station.
	2.	Quadruple	450 41 30	112 40 22,5	
	8.	Sextuple	676 1 30	112 40 22,5	
	4.	Octuple	901 21 30	112 40 22,5	
	5.	Decuple	1126 45 30	112 40 33	

β. *Between Utrecht and Zevenboompjes.*

Date.	No.	Multiples.	Repeated Angles.	Single Angles.	
			° ′ ″	° ′ ″	
1823. 7th Aug. 8h A. M.	1.	Double	209 35	104 47 30	Mean angle 104° 47′ 23″; angle taken from centre of station.
	2.	Quadruple	419 9 30	104 47 22,5	
	3.	Sextuple	628 45	104 47 30	
	4.	Octuple	838 18	104 47 15	
	5.	Decuple	1047 53	104 47 18	

c. Angles taken on the Station of Zevenboompjes.

α. *Between Utrecht and Kooltjesberg.*

Date.	No.	Multiples.	Repeated Angles.	Single Angles.	
			° ′ ″	° ′ ″	
1 823. 7th Aug, 3h P. M.	1.	Double	153 33 30	76 46 45	Mean angle 76° 46′ 33″.3; angle taken from centre of station.
	2.	Quadruple	307 6 30	76 46 37,5	
	3.	Sextuple	460 39	76 46 30	
	4.	Octuple	614 12	76 46 30	
	5.	Decuple	767 44	76 46 24	

β. Between Utrecht and Pyramid.

Date.	No.	Multiples.	Repeated Angles.	Single Angles.	
			° ′ ″	° ′ ″	
1823. 8th Aug. 12^h	1.	Double	111 47	55 53 30	Mean angle 55° 53′ 19″,2; angle taken from centre of station.
	2.	Quadruple	223 33	55 53 15	
	3.	Sextuple	335 20	55 53 20	
	4.	Octuple	447 6 30	55 53 19	
	5.	Decuple	558 52	55 53 12	

γ. Between Kooltjesberg and Amersfoort.

Date.	No.	Multiples.	Repeated Angles.	Single Angles.	
			° ′ ″	° ′ ″	
1823. 8th Aug. 12^h	1.	Double	159 34 30	79 47 15	Mean angle 79° 47′ 16″,1; taken from centre of station.
	2.	Quadruple	319 8	79 47	
	3.	Sextuple	478 44 30	79 47 25	
	4.	Octuple	638 19	79 47 22,5	
	5.	Decuple	797 53	79 47 18	

c. Angles taken on the Steeple of Amersfoort.

α. Between Utrecht and Kooltjesberg.

Date.	No.	Multiples.	Repeated Angles.	Single Angles.	
			° ′ ″	° ′ ″	
1823. 7th Aug.	1.	Double	145 16	72 38	Mean angle 72° 38′ 14″,7; angle reduced to centre of station 72° 37′ 7″,2.
	2.	Quadruple	290 32 30	72 38 7,5	
	3.	Sextuple	435 50	72 38 20	
	4.	Octuple	581 6 30	72 38 19	
	5.	Decuple	726 24 30	72 38 27	

β. Between Utrecht and the Pyramid.

Date.	No.	Multiples.	Rep eated Angles.	Single Angle.	
			° ′ ″	° ′ ″	
1823. 7th Aug.	1.	Double	91 27	45 43 30	Mean angle 45° 43 18″,8; reduced to centre of station 45° 41′ 32″2.
	2.	Quadruple	182 53	45 43 15	
	3.	Sextuple	274 20 30	45 43 25	
	4.	Octuple	365 46	45 43 15	
	5.	Decuple	457 11 30	45 43 9	

γ. *Between Kooltjesberg and Zevenboompjes.*

Date.	No.	Multiples.	Repeated Angles.	Single Angle.
			° ′ ″	
1723. 7th Aug	1.	Double	187 42 30	93 51 15
	2.	Quadruple	375 27	93 51 45
	3.	Sextuple	563 11 30	93 51 55
	4.	Octuple	750 45	93 50 37
	5.	Decuple	938 38	93 51 48
	7.	Duodecuple	1126 28	93 52 20
	7.	Quatuordecuple	1314 6 30	93 51 53

Mean angle 93° 51′ 39″; reduced to centre of station 93° 45′ 7″

a. *Angles taken at Kooltjesberg.*

α. *Between the Steeples of Utrecht and Amersfoort.*

Date.	No.	Multiples.	Repeated Angles.	Single Angles.
			° ′ ″	° ′ ″
1823. 8th Aug.	1.	Double	116 25 30	58 12 45
	2.	Quadruple	232 51	58 12 45
	3.	Sextupie	349 15 30	58 12 35
	4.	Octuple	465 41	58 12 37
	5.	Decuple	582 6 30	58 12 39

Mean angle 58° 12′ 40″,3, from centre of station.

β. *Between the Steeples of Utrecht and Naarden.*

Date.	No.	Multiples.	Repeated Angles.	Single Angles.
			° ′ ″	° ′ ″
1823. 8th Aug.	1.	Double	191 27	95 43 30
	2.	Quadruple	382 54	95 43 30
	3.	Sextuple	574 20 30	95 43 25
	4.	Octuple	765 47	95 43 22
	5.	Decuple	957 13 30	95 43 21

Mean angle 95° 43′ 25″,7, from centre of station.

b. *Between the Steeples of Utrecht and Zevenboompjes.*

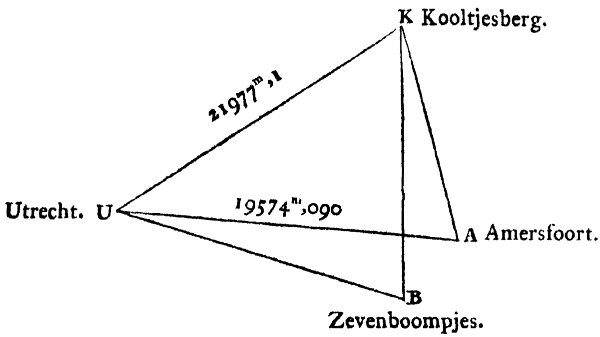
Date.	No.	Multiples.	Repeated Angles.	Single Angles.
			° ′ ″	
1823. 8th Aug.	1.	Double	103 27 30	51 43 45
	2.	Quadruple	206 55	51 43 45
	3.	Sextuple	310 22	51 43 40
	4.	Octuple	413 50	51 43 45
	5.	Decuple	517 16 30	51 43 39

Mean angle 51° 43′ 42″,8, from centre of station.

SECTION IV.

Calculation of the distance between the stations of Kooltjesberg and Zevenboompjes, being the two points where the experiments on the velocity of sound were made.

a. First triangle.



The distance between Utrecht and Amerfoort Steeples from General KRAYENHOFF'S Survey is $19574^{m},090$.

	°	′	″	
Angle UAK =	72	37	7,2	by mensuration.
AKU =	58	12	40,3	
AUK =	49	10	25,2	
	180	0	12,7	
Angle UAK =	72	37	2,96	
AKU =	58	12	36,07	
AUK =	49	10	20,97	

calculating with these angles and UA, the length UK is found equal to $21977^{m},1$.

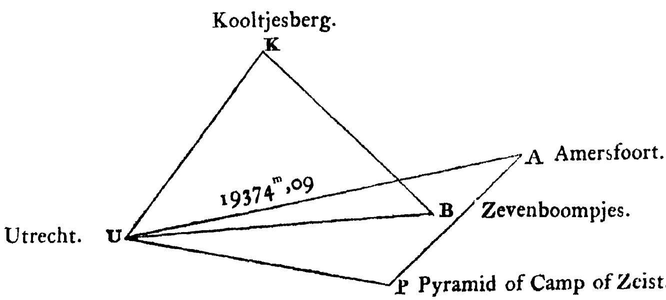
b.

In triangle UBK, we have now UK = 21977^{m},1.

	°	′	″
UBK =	76	46	33,3
BUK =	51	43	42,8
BUK =	51	30	14,1
	180	0	30,2
Thus, UBK =	76	46	23,24
BKU =	51	43	32,73
BUK =	51	30	4,03

With these data we have the distance of the two stations of Kooltjesberg and Zevenboompjes 17668,4 metres.

b. Second triangle.



The distance UA between Utrecht and Amersfoort taken from the Survey of General KRAYENHOFF.

	°	′	″	
UAP =	45	41	32,2	measured angles.
AUP =	21	38	0,5	
UPA =	112	40	20,1	
	179	59	52,8	
Thus UAP =	45	41	34,6	
AUP =	21	38	2,9	
UPA =	112	40	22,5	

Calculating UP the distance from Utrecht to the Pyramid of the Camp of Zeist, we have UP = 15180^m,5 metres.

Again, in triangle U B P we have UP = 15180,5 metres.

< U B P = 55° 53′ 19″,2 U P B = 104 47 23, 1 P U B = 19 18 11, 6	by mensuration for	< K U B = 51° 30′ 14″,1 K U A = 49 10 25, 2	by mensuration.
179 58 53, 9		Substract. 2 19 48, 9 < A U P = 21 38 0, 5	
		< P U B = 19 18 11, 6	

Thus we shall have

$$\begin{aligned} UBP &= 55^{\circ}\ 53'\ 41'',23 \\ UPB &= 104\ \ 47\ \ 45,14 \\ PUB &= 19\ \ 18\ \ 33,63 \end{aligned}$$

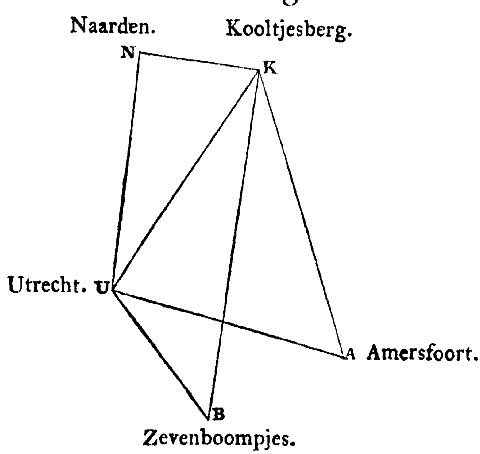
Calculating with these data UB, we have it equal to 17725,8 metres.

But in Δ U B K, we have

$$\left.\begin{aligned} UBK &= 76^{\circ}\ 46'\ 23'',24 \\ BKU &= 51\ \ 43\ \ 32,73 \\ BUK &= 51\ \ 30\ \ \ 4,03 \end{aligned}\right\}\text{found by calculation above.}$$

Therefore we find B K = 17670,85 metres.

c. Third triangle.



In the $\triangle$ U K N

U N = 22987^m,369 metres by General KRAYENHOFF's Survey.

The angle A U N = 61° 23′ 56″,324, measured by General KRAYENHOFF,

$$\begin{array}{lrrr} & {}^{\circ} & {}' & {}'' \\ \text{A U K} = & 49 & 10 & 25{,}200 \\ \hline \angle \text{K U N} = & 12 & 13 & 31{,}124 \\ \angle \text{U K N} = & 95 & 43 & 25{,}\ \ 7 \\ \angle \text{U N K} = & 72 & 3 & 3{,}\ \ 2, \end{array}$$ concluded angle.

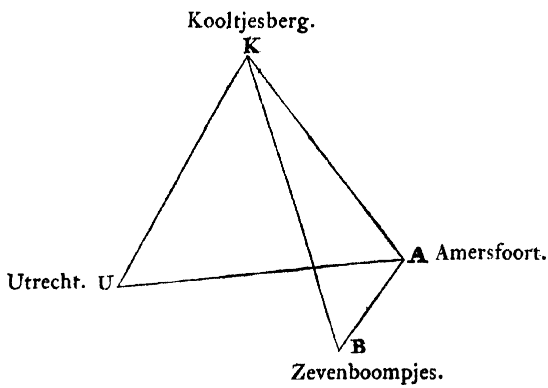
Now calculating U K we have it 21978,2 metres.

Further, in triangle U B K, we have

$$\left.\begin{array}{lrrr} & {}^{\circ} & {}' & {}'' \\ \text{U B K} = & 76 & 46 & 23{,}24 \\ \text{B K U} = & 51 & 43 & 32{,}73 \\ \text{B U K} = & 51 & 30 & 4{,}03 \end{array}\right\}$$ by calculation above-mentioned.

This gives B K = 17669,3 metres.

d. Fourth triangle.



In this triangle, U A K, the distance U A from Utrecht to Amersfoort, is known from General KRAYENHOFF's Survey, 19574,090 metres.

	°	′	″
∠ U A K =	72	37	2,96
A K U =	58	12	36,07
A U K =	49	10	20,97

By these data A K is found equal to 17425,4 metres, the distance from Amersfoort to Kooltjesberg.

In the △ K B A, we have A K = 17425,4 metres.

	°	′	″
∠ K B A =	79	47	16,1
∠ B A K =	93	45	7
∠ A K B =	6	28	57,5
	180	1	20,6

Thus,	K B A =	79	46	49,2
	B A K =	93	44	40,2
	A K B =	6	28	30,6

With these data calculating BK, we have the distance between both stations 17668,55.

By these four different methods of calculation we have the distance between the two points when the observations were made, viz. from Kooltjesberg to Zevenboompjes.

	Metres.
By the 1st method .	17668,40
2d	17670,85
3d	17669,30
4th	17668,55

Mean distance of basis 17669,28 metres, or 9664,7044 English fathoms, the metre being rated at 39,3824 English inches.

SECTION V.

Rate of Clocks with conical Pendulums.

Having shown in the preceding section how the distance between the two stations was ascertained, we must now proceed to show that the clocks with conical pendulums, with which the interval of time between the light and the report was measured, kept a regular rate; for this purpose they were frequently compared on each station with the box chronometers. The following table shows how many decimal seconds and fractions of seconds passed on the conical centrifugal pendulum clock in five minutes of the marine box chronometer. This is the centrifugal clock on the station of Zevenboompjes.

A Table of comparison between the Chronometer and centrifugal Clock at Zevenboompjes.

Chronom. Sexages.	Conical Decimal.	Chron. Sexages.	Conical Decimal.	Chron. Sexages.	Conical Decimal.
′	′	′	′	′	′
5	348,31	5	348,23	5	348,17
	348,10	5	347,85		348,21
	347,85		348,19		348,25
	348,37		348,32		348,19
	348,31		348,28		347,70
	348,70		348,18		348,25
	347,52		347,84		348,18
	348,03		348,26		348,25
	347,78		348,24		348,14
	347,84		348,28		348,42
	348,06		348,16		348,13
	347,94		348,10		348,12
	348,09		348,65		348,22
	348,04		348,37		348,39
	348,08		348,47		348,39
	347,87		348,31		348,34
	348,15		348,18		348,37
	348,10		348,29		348,25
	347,05		348,42		348,30
	347,98		348,23		348,13
	348,13		348,26		348,56
	348,31		348,56		348,21
	348,04		348,34		348,18
	348,20		348,22		348,04
	348,24		348,28		348,31
	348,04		348,36		348,40
	347,93		348,55		348,34
	348,21		348,40		348,10
	347,94		347,63		348,18
	348,15		347,32		348,23

Thus, in 445 sexagesimal minutes by the box chronometer of Zevenboompjes, the index of the centrifugal clock of the same station made 30986,83 turns, and thus one sexagesimal minute on the chronometer was equal to 69″,63 decimal seconds of the conical pendulum clock. The table shows that the conical pendulum clock, during the course of the experiments, did not alter its rate with respect of the chronometer. The comparisons between the clock and the chronometer were made at various times of the day, and immediately before and after the experiments.

At the other station of Zevenboompjes, the box chronometer and conical pendulum clock were frequently compared before, during, and after the experiments. The following table shows that the accuracy of the conical pendulum clock was quite sufficient for the purpose in view.

A Table of comparison, showing how many decimal seconds the conical pendulum clock of the station of Kooltjesberg beat in one sexagesimal minute by a chronometer.

Sexagesimal minute.	Decimal seconds.	Sexagesimal minute.	Decimal seconds.
′	″	′	″
1	69,33	1	69,39
1	69,44		69,68
	69,35		69,56
	69,64		69,22
	69,38		69,44
	69,78		69,45
	69,38		69,23
	69,44		69,47
	69,22		69,70
	69,30		69,38
	69,32		

Thus, in 21 sexagesimal minutes the index of the centrifugal pendulum at Kooltjesberg made 1458,″10 beats.

Thus, one sexagesimal minute on the chronometer of Kooltjesberg, equal to 69,″433 turns of the index of the centrifugal clock of the same station.

SECTION VI.

Experiments on the velocity of sound on the 27th June, 1823, compared with theory.

Having thus far stated the means by which the distance between the stations of Kooltjesberg and Zevenboompjes was ascertained, and the rate determined of the clocks by which the velocity of sound was measured, I will now proceed to give the experiments which were made on the 27th of June, and compare the result with theory. The following table contains the time which sound employed to travel over the basis on the 27th of June, when 22 shots were simultaneously fired, and equally seen and heard on both stations. The first column of this table shows the number of the shot, the second the time which sound employed to travel from Kooltjesberg to Zevenboompjes, as observed on the latter station, and the third column the time which sound employed to come from Zevenboompjes to Kooltjesberg, also observed on the latter place.

Experiments on the velocity of sound, made the 27th June, 1823.

I.	II. Sound travelled from Kooltjesberg to Zevenboompjes.	III. Sound travelled from Zevenboompjes to Kooltjesberg.
	$''$	$''$
1	52,90	51,17
3	52,69	50,89
4	52,71	50,68
5	52,92	50,80
6	52,84	50,86
7	53,04	50,89
8	52,89	51,01
9	52,79	51,00
11	52,83	50,99
12	52,77	50,96
13	52,79	51,10
14	52,99	51,07
16	52,90	51,08
17	52,64	51,28
18	52,90	51,21
19	52,87	51,18
20	52,92	51,33
22	52,91	51,38
23	52,64	51,35
24	52,57	51,32
25	52,90	51,14
26	52,96	51,01
	1162,37	1123,70
	1123,70	
	2286,07	

and $\dfrac{2286,07}{44} = 51,''96.$

Thus, taking the mean of all the observations, we have the velocity with which sound travelled along our basis free of

the accelerating or retarding effect of wind, on the 27th of June, 1823, equal to 51″,96. And as the basis was equal to 17669,28 metres, or 9664,7044 fathoms, we have the velocity of sound, found by experiment as above, equal to 340,06 metres, or 1116,032 English feet per second.

Now, whilst these 22 shots were fired, the mean temperature of the air was

$$\left.\begin{array}{ll} \text{At Zevenboompjes} & = 11^\circ,21 \\ \text{Kooltjesberg} & = 11\ \ 11 \\ \hline \text{Mean temperature on both stations} & \quad 11\ \ 16 \end{array}\right\} \text{Of centigrade scale.} = t$$

The mean altitude of barometer corrected of the effect of capillarity, and reduced to the temperature of 0° of centrigrade scale, was as follows:

Station of Zevenboompjes . .	$0^m,7439$
Kooltjesberg . . .	0 7456
Mean altitude of barometer .	0 74475 $= p$.

The mean tension of aqueous vapour in the atmosphere, as determined by Mr. DANIELL's hygrometer, was at

Station of Zevenboompjes = 0,00901235 metres.

Kooltjesberg = 0,00949378

Mean tension of aqueous vapour, 0,00925307 $= f$.

The effect of gravity, calculated for mean latitude of Amersfoort and Naarden, by the formula

$$g = (g)\ (1 - 0,002837 \cos. 2\ l)$$

$$= \frac{9808,8}{1,0003788 64}\left\{1 - 0,002837 \cos. 2\left\{52^\circ\, 13'\, 33''\, 35\right\}\right\}$$

g = 9812,03 = effect of gravity in lat. 52° 13′ 33″, 35.

The ratio of the specific heat of the air when the volume is constant, to the specific heat of air at a constant pressure, or $\frac{c'}{c}$, is, according to the experiments of GAY LUSSAC and WELTER, equal to $1,3748 = \frac{c'}{c}$.

In Sir ISAAC NEWTON's formula $\sqrt{\frac{gp}{D}}$, by which the velocity of sound is expressed, D is the density of air, that of mercury being taken for unit.

By BIOT's and ARAGO's experiments, the density of perfectly dry air was found at $0^m,76$ barometrical pressure to be equal to unity divided by 10466,82.

But when the barometrical pressure alters and becomes p, and the temperature becomes t, we have by the law of MARIOTTE

$$D = \frac{p}{10466,82 \times 0^m,76 \left\{ 1 + t . 0,00375 \right\}}$$

And introducing into this formula the correction for the aqueous vapour existing in the air, and calling F the tension of aqueous vapour existing in the air, we find

$$D = \frac{p - \frac{3}{8} F}{10466,82 \times 0^m,76 \left\{ 1 + t . 0,00375 \right\}}$$

This value of D being substituted in Sir ISAAC's formula, we have the velocity of sound by theory

$$V = \sqrt{\frac{gp}{D}} = \sqrt{\frac{g p . 10466,82 \times 0^m,76 \left\{ 1 + t . 0,00375 \right\}}{p - \frac{3}{8} F}}$$

$$= \sqrt{\left\{ 10466,82 \times 0^m,76 \left\{ 1 + t . 0,00375 \right\} \right\} \frac{gp}{p - \frac{3}{8} F}}$$

According to LAPLACE, this formula must be multiplied by the square root of the ratio between the specific heat of air at

a constant volume, and the specific heat of air at a constant pressure. Thus the final formula for the velocity of sound, given by theory, is

$$V = \sqrt{\left\{ 10466{,}82 \times 0^{m}{,}76 \left\{ 1 + t \, . \, 0{,}03375 \right\} \right\} \frac{g\,p}{p - \frac{3}{8} F}} \times \sqrt{\frac{c'}{c}}$$

Substituting in this formula the quantities stated above, theory gives the velocity of sound for the state of the atmosphere on the 27th of June, 1823, when the experiments were made, V = 335,14 metres, or 1099,885 English feet; but the velocity as obtained by experiment was 340,06 = 1116,032 feet.

Difference between theory and experiment the 27th of June, 4,92 metres = 16,147 feet.

SECTION VII.

Experiments on the velocity of sound on 28th of June, 1823, compared with theory.

On the 28th of June, 1823, fourteen simultaneous shots were equally seen and heard on both stations; the following table contains the results.

	Number of shots.	Sound travelled from Kooltjesberg to Zevenboompjes in	Sound travelled from Zevenboompjes to Kooltjesberg in
		$''$	$''$
	3	51,81	52,12
	4	51,94	52,10
	5	51,77	51,28
	6	51,98	52,51
	7	52,17	52,46
	8	52,15	52,28
	9	52,25	53,10
	10	52,18	50,17
	12	52,40	52,19
	14	52,27	52,62
	15	52,27	51,66
	17	52,23	51,52
	18	52,49	51,99
	19	52,56	51,60
Sum		730,47	727,60

The mean result by experiment on the 28th of June, 1823, is $\frac{730'',47+727'',60}{28} = 52,07$, in which time sound travelled along the basis of 17669,28 metres, or 57988,2264

English feet. Thus, the mean velocity of sound on the 28th of June in 1″, is 339,34 metres = 1113,669 English feet.

The mean temperature, when these experiments were made, was at

Zevenboompjes - - -	11°,07	Centigrade scale.
Kooltjesberg - - - -	11 36	
Mean temperature - -	11 215 = t	

Mean height of the barometer, corrected for capillarity, and reduced to 0° of centrigrade scale,

Zevenboompjes - - - -	0,7476 metres.
Kooltjesberg - - - - -	0,7487
Mean barometer, or p =	0,74815
Mean tension of aqueous vapour by Mr. DANIELL's hygrometer - - F =	0,00840465

These quantities being substituted in the formula, we have the velocity of sound, by theory, on the 28th of June, 1823, V = 335,^{m}10 metres = 1099,753 English feet; by experiment, 339^{m},34 metres = 1113,669 feet.

Difference between theory and experiment 4,24 metres = 13,916 feet.

Thus it appears by the experiments both of the 27th and 28th of June, that sound travels faster than its theoretical calculation.

The 27th of June, difference of experiment and theory 4^{m},92
28th of June - - - - 4^{m},24

The difference between the experiments of 27th and 28th of June, is but of 0^{m},62, or 2,3629 feet; that is about $\frac{1}{472}$ of the mean result of the experiments of both days.

The French philosophers found a difference between their experiments of 23d and 24th of June, 1822, of $\frac{1}{90}$. But the difference of $\frac{1}{472}$, which we obtained, if we reduce the observations of both days to what they would have been in perfectly dry air, and in temperature of 0° *cent.* is still remarkably lessened. The formula by which the velocity of sound in given hygrometrical circumstances, and a given temperature of the air, is reduced to what it would be in dry air of 0° *cent.* temperature, calling U′ the velocity of sound in dry air of 0° temperature; U the velocity of sound at a tension of aqueous vapour = F, is as follows:

$$U' = \frac{U}{\sqrt{\left\{1 + 0{,}00375\,t\right\}}} \times \sqrt{\left\{1 - 0{,}37651\right)\frac{F}{p}}.$$

The 27th of June, 1823, we had

$U = 340^{m},06 = 1116{,}032$ English feet

$t = 11^{\circ},16$ *cent.*

$F = 0{,}00925307$

$p = 0{,}74475$ metres.

Substituting these quantities in the formula, we have

$U' = 332^{m},38 = 1090{,}827$ English feet.

The 28th June, 1823, we had

$U = 339^{m},34 = 1113{,}669$ feet

$t = 11^{\circ},215$

$F = 0{,}00840465$;

which being substituted in the formula, we have

$U' = 331^{m},72 = 1088{,}661$ English feet.

Thus the difference between the observations of both days, when reduced to dry air, and 0° *cent.* is $0^{m},66 = 2{,}166$ feet; or $\frac{1}{503}$ of the mean of the observations of both days. It appears also, that by our experiments of the 27th and 28th

of June, 1823, the mean velocity of sound in air perfectly dry, and at 0° temperature, was $332^{m},05 = 1089,744$ feet in a second.

SECTION VIII.

Experiments on the 25th of June, when the shots were not reciprocal.

The following experiments will I trust prove, that in experiments on the velocity of sound, such observations can only be relied on in which the shots on both stations were reciprocal, that is fell within the same second in both places, and were equally heard and seen on both stations. The 25th of June, the cannon fired at Zevenboompjes was not heard at Kooltjesberg, but at Zevenboompjes the report of the guns fired at the other place was distinctly perceived. The following table shows the time preterlapsed between the light and report, as observed at Zevenboompjes.

	Number of Shots.	Time between light and report.	
1823. 25th June.	1.	52,31″	Observations at the station of Zevenboompjes, guns fired at the station of Kooltjesberg.
	2.	52,59	
	4.	52,47	
	7.	52,20	
	8.	52,47	
	10.	52,17	
	12.	52,27	
	14.	52,52	
	15.	52,54	
	16.	52,43	
	17.	51,91	
	19.	52,50	

Sum 628,39, which being divided by twelve, the number of observations, gives the passage of

sound along the basis $52'',37$. Thus the mean velocity in $1'$ was $337^m,39 = 1107,268$ feet.

The mean temperature at the time of these experiments

$$\left.\begin{array}{ll} \text{at Zevenboompjes} & 7^\circ,41 \\ \text{Kooltjesberg} & 8\ ,54 \\ \text{Mean temperature of the air,} & 7\ ,975 = t \end{array}\right\}\text{centigrade}$$

Mean height of the barometer corrected for capillarity, and at 0° *cent.*

at Zevenboompjes	$0^m,7522$
Kooltjesberg	$0\ ,7538$
Mean barometer - - -	$0\ ,7530 = p$

Mean tension of aqueous vapour in the air,

at Zevenboompjes	0,00737444
at Kooltjesberg -	0,00706966.
Mean tension - -	$0,00722205 = F$,

which quantities substituted in the formula, we have for temperature 0° *cent.* and in perfectly dry air the velocity of sound $U' = 331,85$ metres $= 1089,087$ feet.

SECTION IX.

Experiments of 26th of June, 1823, when the shots were not reciprocal.

The 26th of June, the following shots were seen and heard at Kooltjesberg and fired at Zevenboompjes, but no shots from the first station were heard at the latter.

	Number of Shots.	Time between light and report.	
	1.	50,20″	Guns fired at Zevenboompjes, heard and seen at Kooltjesberg.
	2.	50,80	
	3.	51,44	
	4.	52,20	
	5.	51.10	
	9.	50,11	
	11.	50,99	
	12.	50,81	
	13.	51,00	
	14.	51,01	
	16.	51,12	
Total of 12 shots		560,78	The mean of which is 50″98

which gives a velocity of 346,59 metres = 1137,134 feet in 1″.

The temperature was at that time

at Zevenboompjes 11°, 57
Kooltjesberg 12 , 54

Mean temperature . . 12,055 = t.

Mean atmospheric pressure at Zevenboompjes $0^m,7493$.
Mean atmospheric pressure at Kooltjesberg $0^m,7512$.
Mean pressure of atmosphere $0,75025 = p$.
Mean tension of aqueous vapour at Zevenboompjes } 0,00892922
at Kooltjesberg 0,01011376
Mean tension of aqueous vapour . . $0,00952149 = F$

Calculating by this datum we shall have the observed velocity of sound in 1″ reduced to dry air and 0° temperature $V = 338^m,20 = 1109,927$ feet; but the experiments of the 25th gave $V' = 331,^m85 = 1089,087$ feet. Difference $6^m,35 = 20,840$ feet per 1″ between the experiments of the 25th and 26th of June, in which the shots were not reciprocal. This difference is about $\frac{1}{53}$ of the mean of both observations. But the 27th and 28th of June, when the shots were reciprocal, the difference between the results of both days were only $0^m,66 = 2,166$ feet, that is about $\frac{1}{503}$ of the mean result of the observations.

From the comparison of these results we may safely infer, that only such shots will answer the purpose for which these experiments are made, which are exactly fired at the same instant on both stations.

It is in this respect, I imagine, that our experiment may claim some attention, as the very great care and ability of our artillery men enabled us have the guns fired within the interval of one second.

A Table showing the results of experiments on the velocity of sound as observed by different philosophers.

Names of Observers.	Time when made.	Country where made.	Length of basis. metres.	Velocity of Sound per Second in metres.	
Mersenne		France		448	1
Florentine Philosophers	1660	Italy	1800	361	2
Walker	1698	England	800	398	3
Cassini, Huigens, &c.		France	2105	351	4
Flamsteed and Halley		England	5000	348	5
Derham	1704 & 1705	England	1600 à 2000	348	6
French Academians	1738	France	22913 & 28526	332,93 at 0° c	7
Blanconi	1740	Italy	24000	318	8
La Condamine	1740	Quito	20543	339	9
La Condamine	1744	Cayenne	39429	358	10
T. F. Mayer	1778	Germany	1040	336,86	11
G. E. Muller	1791	Germany	2600	338	12
Epinoza and Banza	1794	Chili	16345	356,14 at 0°	13
Bénzenberg	1809	Germany	9072	333,07 at 0°	14
Arago, Mathieu Prony	1822	France	18612	331,05 at 0°	15
Moll, Van Beek, and Kuytenbrouwer	1823	Netherlands	17669,28	332,05 at 0° and dry air.	16

1. Mersenne de Arte Ballistica Prop. 39.
2. Tentamina Experim. Acad. del. Cimento, L. B. 1738, Part II. p. 116.
3. Philos. Trans. 1698, No. 247.
4. DUHAMEL, Hist. Acad. Reg. L. II. Sect. 3, Cap. II.
5. Philos. Trans. 1708 and 1709.
6. Ibid, ibid.
7. Mem. de l'Academie des Sciences, 1738 and 1739.
8. Comment. Bononienses, Vol. II. p. 365.
9. La CONDAMINE Introduction Historique, &c. 1751, p. 98.
10. Mem. de l'Acad Royale des Sciences, 1745, p. 488.
11. J. T. MAYER, Praktische Géométrie Göttingen, 1792, B. 1. p. 166.
12. MULLER, Götting. Gelehrt. Anzeige, 1791, St. 159 & Voigts Magazin, &c. B. 8, St. 1. p. 170.
13. Annales de Chimie. et de Phys. T. VII. p. 93.
14. GILBERT'S Annalen, neue Folge, B. V. p. 383.
15. Connaissance des Tems. 1825, p. 361.

Communicated by Captain H. KATER, *F. R. S.*

XXIV. *A Catalogue of nearly all the principal fixed Stars between the zenith of Cape Town, Cape of Good Hope, and the South Pole, reduced to the 1st. of January, 1824. By the Rev.* FEARON FALLOWS, *M. A. F. R. S.*

Read Feb. 26, 1824.

THE following Catalogue of nearly all the principal fixed stars in the southern hemisphere, from the zenith of Cape Town to the South Pole, was deduced from observations made during the latter part of 1822 and the beginning of the present year. Its pretensions to accuracy will be easily estimated by stating the circumstances under which the observations were taken, the respective merits of the instruments used, and the attention, on the part of the observer, to do every justice to the means placed in his power. Immediately after my arrival in this colony (at the end of 1821,) I lost no time in personally examining different parts of the country, for the purpose of selecting one, which might be deemed eligible as a site for the intended Observatory about to be erected here. After many fruitless endeavours to accomplish the object of my wishes, I had the good fortune, at length, to find a situation in the vicinity of Cape Town, which, upon the whole, possessed more local advantages than any I had seen elsewhere. My Report, containing a description of this site, and a Map of the surrounding country, was forwarded to My Lords Commissioners of the Admiralty in the month of

March, 1822: since then I have had no reason to change my opinion upon the propriety of my choice.*

As a considerable period would likely intervene between the date of my Report, and the time when instructions would be received to commence the building of the Observatory, I was desirous of employing this interval in forming a Catalogue of fixed stars which might prove useful, when more extensive means of accomplishing the work with greater truth might be placed in my power, I therefore lost no time in requesting His Excellency Sir RUFANE DONKIN, the Acting Governor, to allow me a small wooden house, which could be easily converted into a temporary Observatory. My request was kindly granted; and the necessary alterations soon made for the reception of a portable transit instrument. a clock, and an altitude and azimuth circle.

It is not my intention to enter into any minute detail of the disposition of the instrument in this confined apartment, which would be tedious and uninteresting; but I think it right to give a short description of each instrument, and the methods employed for insuring a near approximation to accuracy.

The transit instrument by which the right ascensions in the following Catalogue were deduced is of very diminutive size, though excellent of its kind. It was made by Mr. DOLLOND, and fitted up in his usual manner with every conve-

* I am much indebted to the kindness of my learned friend Mr. COLEBROOKE, (then on a visit here), whose knowledge of the Cape is very extensive, and by whose advice and assistance I was enabled to make up my mind upon the advantages of *each spot* without much delay.

nience for the necessary adjustments, and for illuminating the wires in the eye-piece. The focal length of the object-glass is about 19½ inches; its diameter 1,62 inches. The frame work is made of cast-iron; the lower part of which is securely fastened to a block of stone, and this again strongly cemented to a firm brick pillar. The only defect which has yet been noticed in the frame is its liability to be affected by the variation of temperature. We obviate this in some measure, by constantly* levelling the axis of the telescope whenever an opportunity presents itself during a nights' observations.

The method of placing a transit in the plane of the meridian by a star near the Pole, so successfully employed in the northern, fails in the southern hemisphere, at least in *small* instruments; as I am not aware of any star within 12 degrees of the South Pole that can be classed higher than the fifth magnitude, and which of course can be very seldom observed at its superior and inferior culmination. I therefore deemed it advisable to take the transits of as many high and low Greenwich stars as possible, by which means, as is well known, the azimuthal error of the instruments may be easily ascertained, and the error of the clock to much nicety. Lest any change might take place in the *position* of the transit between two successive nights' observation (though correctly verified by a distant mark during the *day*), I determined the

* It is worth while to remark, that the lamp for illuminating the wires ought to be lighted some time *before* the axis is levelled, and the instrument prepared for work, as the heat of the lamp communicating with the metallic frame will derange the level, and consequently render the observations of no use. This remark applies only to portable transit instruments.

rate of the clock by the transits of stars near the zenith, as the azimuthal error would hardly be felt when the axis was correctly levelled. The rates thus obtained were proportioned to the intervals between the high and low Greenwich stars, and the error of position easily found; afterwards, the observed transits of other stars were corrected to the meridian according to their respective altitudes or declinations, by a method well known to all practical astronomers.

By uniting the results of several pairs of Greenwich stars taken in the same night, the error of position is more correctly found.

For example. Nov. 19, 1822.

$$\text{The azimuthal error} = \begin{cases} 0^s{,}705 & \text{by Fomalhaut and } \alpha \text{ Androm.} \\ 0{,}804 & \text{Fomalhaut and } \alpha \text{ Arietis.} \\ 0{,}711 & \text{Sirius and Capella.} \end{cases} \quad \text{East.}$$

Mean 0,74 East.

Nov. 26, 1822.

$$\text{The azimuthal error} = \begin{cases} 0^s{,}05 & \text{by Fomalhaut and } \alpha \text{ Androm.} \\ 0{,}47 & \text{Fomalhaut and } \alpha \text{ Arietis.} \\ 0{,}54 & \text{Sirius and Capella.} \end{cases} \quad \text{East.}$$

"The mean of the two last (viz. $0^s{,}50$) was preferred, as the lamp had only been lit a short time before the observation of α Andromedæ, and the level was found deranged."

Having briefly pointed out the plan by which the right ascensions in the following Catalogue were obtained, I shall now put down a few observations of two bright stars, as specimens of the rest, *corrected* for the error of the instrument,

the error and rate of the clock, but *uncorrected* for the effects of precession, aberration, lunar and solar *nutation.*

Apparent right ascensions of Achernar.

		H.	M.	S.
1823, Nov.	1	1	31	9,54
	7	1	31	9,39
	12	1	31	9,31
	15	1	31	9,38

Apparent right ascensions of Canopus.

		H.	M.	S.
1822, Oct.	30	6	20	2,91
Nov.	7	6	20	3,39
	15	6	20	3,27
	19	6	20	2,97
	28	6	20	2,93
1823, Jan.	24	6	20	3,00
	27	6	20	3,11
	23	6	20	3,66

The mean right ascension of Canopus, reduced to the 4th of January, 1822, by observations made at the beginning of the year, is $6^h\ 20^m\ 00^s,00$, and by those at the end of the year $6^h\ 19^m\ 59^s,95$.

The clock, which stands at a short distance from the transit, does not go so well as could be wished. After a great deal of lost labour, and by a careful examination of its parts, I succeeded in remedying several defects, and at length brought it to have a nearly equable rate. The dampness of the apartment, especially after rain, *may* have a sensible influence upon the going of the clock, which no artist could provide against.

The altitude and azimuth circle was made by Mr. RAMSDEN, and originally used as an equatorial instrument. The diameter of the vertical circle is 30 inches. The telescope, with two micrometers attached to the object and eye ends revolves, the circle itself being fixed to the upright axis. The frame is made of iron, and strongly screwed to a large block of stone (weighing about two tons) imbedded in the ground, and resting upon a rock. I have not yet had occasion to use the azimuth circle, except merely for reversing the face of the vertical circle. One cause of imperfection in this instrument is the slight manner in which the microscopes are connected with the telescope, the supports being too weak, and consequently liable to bend and be thrown out of adjustment in elevating or depressing the telescope to any given object. I have felt this defect severely, but could not get it *altogether* remedied here; still however I have some hopes (from the great care and caution in the management of the instrument) that the declinations in the following Catalogue will not be found very inaccurate.

The latitude of the Observatory was principally obtained from a great number of observations of α Aquilæ (this star having nearly the same zenith distance at Greenwich and the Cape), and compared with its north polar distance, as given by Mr. POND in the Nautical Almanack for 1822. I tried several other Greenwich stars, and found a slight difference of a few seconds in the results, some in excess, others in defect; but this is no doubt the fault of my circle: it is however satisfactory to know, that the mean of all agreed to nearly one second with that deduced from α Aquilæ. Whatever error I may hereafter find in my latitude will of course

effect the declinations in the Catalogue. I was compelled to use this method of finding the latitude, as it was impossible, with my present means, to discover the altitude of the Pole by observations of circumpolar stars at their superior and inferior transits.

A considerable number of the stars in the Catalogue are *double*: the place in the brighter is always given. I cannot find that M. DE LA CAILLE mentions a star of nearly the fourth magnitude accompanying α Crucis (see No. 139).

To the magnitudes, I have sometimes annexed the signs + or —; the meaning of which is this; that if a star appear hardly of the fourth magnitude we set it down 4—; if a little greater than a fourth, 4+; and so on of others.

It must be remarked, that the apparent altitudes of stars were always reduced to the true by Dr. YOUNG'S tables of refractions, inserted in the Nautical Almanacks each year.

Formulæ, by which the apparent right ascensions and declinations were reduced to the beginning of the year,*

$$-\delta R = \left.\begin{aligned} &(+\overset{s.}{3{,}0678} - \overset{s.}{1{,}336}\ \text{sin. R. tang. D}),\ t \\ &-1{,}\ 239\ \text{cos. R. sec. D. cos.}\ \odot \\ &-1{,}\ 350\ \text{sin. R. sec. D. sin.}\ \odot \\ &+0{,}\ 643\ \text{cos. R. tang. D. cos.}\ \Omega \\ &+0{,}4788\ \text{sin. R. tang. D. sin.}\ \Omega \\ &-1{,}\ 103\ \text{sin.}\ \Omega \\ &+0{,}0289\ \text{sin. R. tang. D. sin.}\ 2\ \odot \\ &+0{,}0265\ \text{cos. R. tang. D. cos.}\ 2\ \odot \\ &-0{,}06115\ \text{sin.}\ 2\ \odot \end{aligned}\right\}$$

* The stars were all reduced to the beginning of the year in which the observations were made, and afterwards brought up to the 1st. of January, 1824, by the quantity of annual precession.

$$
\left.
\begin{aligned}
-\delta D = &-20'',044 \cos. R\,.\,,t \\
&-20,255 \cos. R\,.\sin. D\,.\sin. \odot \\
&+18,580 \sin. R\,.\sin. D\,.\cos. \odot \\
&-\ \ 8,0659 \cos. D\,.\cos. \odot \\
&-\ \ 9,648 \sin. R\,.\cos. \Omega \\
&+\ \ 7,1822 \cos. R\,.\sin. \Omega \\
&+\ \ 0,3982 \cos. R\,.\sin. 2\odot \\
&-\ \ 0,434 \sin. R\,.\cos. 2\odot
\end{aligned}
\right\}
$$

No.	Names and Magnitudes of Stars.		La Caille's Magnitudes.	Right Ascension. 1824, Jan. 1.	Annual Precession.	South Declination.* 1824, Jan. 1.	Annual Precession.
				h. m. s.	s.	° ′ ″	″
1	ι Phœnicis	4	4	0. 0.26.6	+3.13	46.43.10	−20.05
2	γ 3 Octantis	5	5	0. 1.46.2	+2.98	83.12.14	20.05
3	ε Toucanæ	5	5	0.10.49.4	+2.93	65.56.32	20.03
4	β Hydri	3+	3	0.16.17.1	+2.61	78.14.45	20.00
5	κ Phœnicis	5	5	0.17.30.8	+2.96	44.39.30	19.99
6	α Phœnicis	2	2	0.17.33.6	+2.97	43.15.49	19.99
7	λ 1 Phœnicis	5	5	0.22.53.4	+2.91	49.45.50	19.95
8	β 1 Toucanæ	4	4	0.23.26.1	+2.79	63.56. 2	19.94
9	β 2 Toucanæ	4−	4	0.23.26.4	+2.80	63.56.24	19.94
10	β 3 Toucanæ	5−	5	0.24.40.1	+2.80	64. 0.11	19.93
11	μ Phœnicis	5	5	0.32.59.4	+2.86	47. 3. 1	19.84
12	σ Phœnicis	5	5	0.35.23.9	+2.74	58.25.46	19.81
13	β Phœnicis	3.4	4	0.58.12.9	+2.70	47.39.42	19.40
14	ζ Phœnicis	5	5	1. 0.57.7	+2.55	56.11.10	19.34
15	γ Phœnicis	3.4	3	1.20.42.5	+2.62	44.13.21	18.82
16	δ Phœnicis	4−	4	1.23.54.7	+2.50	49.59.14	18.72
17	Achernar	1	1	1.31. 8.6	+2.24	58. 8. 7	18.48
18	χ Eridani	4	4	1.49. 5.7	+2.27	52.29.25	17.82
19	σ 2 Hydri	4.5	5	1.50.28.8	+1.49	68.31. 1	17.76
20	α Hydri	3−	3	1.53.14.0	+1.86	62.25.35	17.65
21	φ Eridani	4	4	2.10.13.0	+2.14	52.19.53	16.90
22	δ Hydri	4	4	2.18.37.7	1.04	62.27.52	16.49
23	κ Eridani	4.5	5	2.20.30.0	2.20	48.29.59	16.40
24	σ Eridani	5	5	2.33. 5.0	2.28	43.39. 2	15.74
25	ι Eridani	4	4	2.33.40.9	2.37	40.36.50	15.70
26	ζ Hydri	5+	5	2.36.53.7	0.87	69. 1.26	15.53
27	ϑ Hydri	5	5	2.42.51.6	0.88	68.21.21	15.19
28	θ 1 Eridani	4	3	2.51.35.6	2.28	41. 0.53	14.69
29	θ 1 Hydri	5−	5	3. 1.57.2	0.03	72.35.16	14.05
30	*l* Eridani	4.5	4	3.12.52.0	2.12	43.45. 0	13.36
31	*y* Eridani	5+	5	3.30.47.1	2.15	40.51.26	12.15
32	*h* Eridani	5	5	3.36.16.7	2.23	37.52.23	11.76
33	β Reticuli	4	4	3.42. 1.2	0.67	65.21.55	11.35
34	*f* 2 Eridani	5+	4	3.42. 5.7	2.21	38. 9.52	11.35
35	*g* Eridani	5	4	3.42.52.5	2.25	36.44.20	11.29
36	*i* Eridani	5	5	3.46.55.3	+2.28	35.15.29	11. 0
37	γ Hydri	3−	3	3.50. 4.0	−1.06	74.46.42	10.76
38	δ Reticuli	5−	5	3.55.58.5	+0.92	61.53.45	10.33
39	γ Reticuli	5	5	3.58.20.6	0.84	62.39. 9	10.15
40	α Horologii	5+	5	4. 8.10.5	1.98	42.44. 1	9.40
41	X Eridani	3.4	4	4.11.14.0	2.26	34.14.10	9.16
42	γ Doradûs	4	4	4.11.24.7	1.55	51.56.27	9.15
43	α Reticuli	3.4	3	4.12.10.4	0.77	62.54.45	9.09
44	ι Reticuli	5+	5	4.13.27.6	1.03	59.43.46	8.99
45	θ Reticuli	5−	5	4.15.41.7	0.65	63.41. 2	8.81
46	*d* Eridani	5	4	4.17.25.5	2.25	34.25.57	8.68
47	σ Reticuli	5−	5	4.20. 0.6	0.61	63.48.21	8.47
48	δ Cœli Scalpt	5	5	4.25.26.0	+1.83	45.20.12	− 8.04
				h. m. s.		° ′ ″	

* N. B. All the declinations in my Catalogue must be diminished four seconds. (Extract of a Letter from the Rev. F. FALLOWS, to JOHN BARROW, Esq. Sec. Admiralty, dated Cape Town, Jan. 10, 1824.)

No.	Names and Magnitudes of Stars.		La Caille's Magnitudes.	Right Ascension.	Annual Precession.	South Declination.*	Annual Precession.
				h. m. s.	S	° ′ ″	
49	α Doradûs	3	3	4.30.11.8	+1.28	55.24.39	− 7.66
50	α Cœli Scalpt.	5+	5	4.34.53.7	1.94	42.12.21	7.27
51	β Cœli Scalpt.	5	5	4.35.49.7	2.12	39.29.44	7.23
52	γ 1 Cœli Scalpt	5	5	4.58. 4.7	2.15	35.43.42	5.35
53	ε Columbæ	4	4	5.24.58.2	2.12	35.36.24	3.06
54	β Doradûs	4	4	5.32. 6.1	0.51	62.36.24	2.44
55	α Columbæ	2.	2	5.33.16.3	2.17	34.10.21	2.34
56	δ Doradûs	5	5	5.44.27.3	0.11	65.48. 9	1.36
57	β Columbæ	3	3	5.44.45.7	2.11	35.50.21	1.34
58	ε Doradûs	5	5	5.50. 3.3	−0.07	66.57. 3	0.87
59	γ Columbæ	4−	4	5.51.18.1	+2.15	38.18.31	0.77
60	σ Columbæ	5+	5	5.53.45.4	+1.83	42.49.53	0.55
61	θ Columbæ	5	5	6. 1.29.7	+2.06	37.14.13	+ 0.12
62	Κ Columbæ	5	5	6.10.17.8	2.13	35. 5.16	+ 0.89
63	Canopus	1	1	6.20. 2.6	1.33	52.36.21	+ 1.75
64	ν Argûs	3	3	6.32.22.2	1.83	43. 2.52	2.82
65	τ Argûs	4	4	6.45.34.0	1.48	50.24.32	3.96
66	α Equulei Pict.	4	4	6.46.22.7	0.63	61.45.19	4.03
67	A Argûs in pup.	5	5	7. 2.56.8	2.01	39.22.43	5.44
68	J Argus in pup.	5	5	7. 7.32.6	1.72	46.28.16	5.82
69	L 1 Argus in pup.	5	5	7. 7.57.6	+1.79	44.52.51	5.86
70	γ Piscis Volantis	5	5	7.10.12.1	−0.47	70.12.47	6.04
71	π Argûs	3	3	7.10.55.6	−2.12	36.47.19	6.11
72	δ Piscis Volantis	5+	5	7.16.53.5	−0.00	67.38. 3	6.60
73	σ Argûs	4+	4	7.23.39.3	+1.91	42.57. 4	7.16
74	c Argûs in pup.	5	5	7.38.59.3	2.13	37.32.56	8.39
75	P Argûs in pup.	4.5	5	7.43.52.5	1.83	45.56. 6	8.78
76	a Argûs in pup.	5	5	7.46.10.2	2.06	40. 7.38	8.96
77	b Argûs in pup.	5	5	7.46.25.3	2.12	38.24.52	8.98
78	R Argûs in pup.	5	5	7.48. 8.0	1.76	47.38.59	9.11
79	χ Argûs	3	4	7.52.18.1	1.53	52.30.58	9.44
80	ϛ Argus	2−	2	7.57.24.3	2.11	39.30.51	9.82
81	γ 1 Argûs	5+		8. 4. 4.1	1.85	46.49.54	10.33
82	γ 2 Argûs	2	2	8. 4. 6.7	1.85	46.49.25	10.33
83	ε Piscis Vol.	5	5	8. 7.19.2	0.24	68. 6. 9	10.57
84	g Argûs in pup.	5	5	8.11.58.4	2.25	36. 7.15	10.91
85	ε Argûs	2−	2	8.18.53.7	+1.24	58.56.55	11.49
86	α Chamæleontis	5+	5	8.22.55.1	−1.40	76.21.47	11.70
87	σ Piscis Vol.	5−	5	8.23.33.8	−0.44	72.50. 2	11.75
88	β Piscis Vol.	5	5	8.23.47.7	−0.69	65.32.58	11.77
89	θ Chamæleontis	5−	5	8.25.43.5	−1.56	76.54.55	11.90
90	β Naut. Pixidis	5	5	8.33.13.0	+2.34	34.41.24	12.42
91	b Argûs in Velis	5	5	8.34.47.8	1.99	46. 1.39	12.53
92	o Argûs	4	4	8.35.15.4	1.72	52.18. 6	12.56
93	d Argus in Carina	5	5	8.36.43.4	1.33	59. 8.16	12.66
94	δ Argûs	3+	3	8.39.50.7	+1.65	54. 4.13	12.87
95	σ Chamæleontis	5	5	8.47. 4.2	−1.75	78.19.27	13.35
96	822 Chamæleontis	5−	6	8.48.57.9	−1.76	78.25.32	+13.47
				h. m. s.		° ′ ″	

* N. B. All the declinations in my Catalogue must be diminished four seconds. (Extract of a Letter from the Rev. F. FALLOWS, to JOHN BARROW, Esq. Sec. Admiralty, dated Cape Town, Jan. 10, 1824.)

No.	Names and Magnitudes of Stars.		La Caille's Magnitudes.	Right Ascension.	Annual Precession.	South Declination.*	Annual Precession.
				h. m. s.	S.	° ′ ″	
97	*b* 1 Argûs in Carina	5	5	8.52.39.9	+1.47	58.33.14	+13.71
98	*b* 2 Argûs in Carina	5	5	8.55. 5.1	1.50	58.24.51	13.86
99	c Argûs in Vel.	5	5	8.58. 5.7	2.07	46.24.12	14.05
100	*a* Piscis Vol.	5	5	8.59.38.5	0.97	65.41.49	14.15
101	λ Argûs	3+	3	9. 1.32.1	2.21	42.43.40	14.27
102	G. Argûs in Carina	5	5	9. 4.36.6	0.24	71.53.50	14.45
103	*a* Argûs in Carina	5	5	9. 6.19.9	1.58	58.15.14	14.56
104	*i* Argûs in Carina	5	5	9. 7.16.4	1.38	61.36. 0	14.61
105	β Argûs	2+	1	9.11.14.5	0.75	68.59.51	14.85
106	ι Argus	2	2	9.12.21.0	1.61	58.32.29	14.92
107	κ Argûs	3+	3	9.16.40.2	1.85	54.15.54	15.17
108	*n* Argûs in Carina	5	5	9.22.57.8	1.32	64.10.19	15.52
109	ψ Argûs	4—	4	9.23.46.0	2.37	39.42. 4	15.56
110	N Argûs in Vel.	5	5	9.25.51.3	1.82	56.15.47	15.68
111	*h* Argûs in Carina	5	5	9.29.20.7	1.74	58.27. 0	15.87
112	*l* Argûs in Carina	5	5	9.40.23.2	1.65	61.42. 1	16.44
113	υ Argûs	3.4	3	9.42.42.0	1.51	64.15.30	16.55
114	φ Argûs	4—	4	9.50.41.8	2.10	53.44. 5	16.94
115	*q* Argûs in Vel.	4—	4	10 7.22.4	2.52	41.15.14	17.67
116	ω Argûs	4.5	4	10. 9.32.2	1.44	69.10.10	17.76
117	*q* Argûs in Carina	5	5	10.11.11.7	1.99	60.27.29	17.83
118	T Argûs in Vel.	5	5	10.14.21.2	2.21	55. 9.39	17.95
119	*r* Argûs in Vel.	5—	5	10.14.48.4	2.56	40.46.17	17.97
120	J Argûs in Carina	5	5	10.20.51.0	1.22	73. 8.24	18.20
121	*p* Argûs in Carina	4	4	10.25.47.9	2.11	60.47. 2	18.37
122	*p* Argûs in Velis	5+	5	10.29.55.7	2.51	47.18.58	18.52
123	γ Camæleontis	5	5	10.33.17.2	0.81	77.41.50	18.63
124	θ 1 Argûs	5		10.35.59.1	2.11	63.33. 0	18.71
125	θ 2 Argûs	2.3	3	10.36.40.2	2.12	63.28.27	18.73
126	σ Argûs	2	2	10.38.16.2	2.30	58.45.47	18.78
127	μ Argûs	3	3	10.39.13.1	2.55	48.29.19	18.81
128	δ 2 Chamæleontis	5	5	10.44. 0.3	0.69	79.36.53	18.95
129	μ Argûs in Carina	5	5	10.46.24.0	2.39	57.55.20	19.02
130	π Centauri	4	4	11.13. 0.2	2.70	53.31.51	19.62
131	λ Centauri	4+	4	11.27.41.2	2.71	62. 2.55	19.84
132	ε Chamæleontis	5	5	11.50.57.0	2.84	77.14.39	20.03
133	σ Crucis	4.5	5	11.57.47.0	3.04	63.38. 1	20.04
134	δ Centauri	3	3	11.59.17.2	3.06	49.44.40	20.04
135	ε Centauri	4	4	12. 2.28.7	3.09	51.23.31	20.04
136	δ Crucis	3—	3	12. 5.49.8	3.12	57.46.19	20.04
137	β Chamæleontis	5	5	12. 8.19.6	3.30	78.20.18	20.03
138	ε Crucis	4	4	12.11.54.0	3.19	59.25.48	20.02
139	α 1 Crucis	4—		12.16.46.4	3.25	62. 8.57	20.00
140	α 2 Crucis	1	1	12.16.51.6	3.25	62. 7.32	20.00
141	σ Centauri	5+	5	12.18.34.7	3.19	49.15. 6	19.98
142	γ Crucis	2.3	2	12.21.28.5	3.25	56. 7.29	19.96
143	γ Muscæ	4—	4	12.22. 6.8	3.44	71. 9.35	19.95
144	α Muscæ	4	4	12.26.48.5	+3.46	68. 9.51	+19.91
				h. m. s.		° ′	

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No.	Names and Magnitudes of Stars.		La Caille's Magnitudes.	Right Ascension.	Annual Precession.	South Declination.*	Annual Precession.
				h. m. s.	S.	° ′ ″	
145	τ Centauri	5+	5	12.28. 7.3	+3.25	47.34.14	+19.89
146	γ Centauri	3—	3	12.31.52.1	3.27	47.59.26	19.85
147	β Muscæ	4—	4	12.35.37.0	3.56	67. 8.36	19.80
148	β Crucis	2+	2	12.37.31.4	3.43	58.43.28	19.77
149	N Centauri	5+	5	12.43.44.7	4.18	39.13. 4	19.68
150	o Centauri	5	5	12.44.19.4	3.45	56.13.11	19.67
151	δ Muscæ	4—	4	12.50.18.6	3.89	70.35.49	19.56
152	ξ 2 Centauri	5	5	12.56.42.4	3.44	48.57.33	19.43
153	ι Centauri	3+	3	13.10.45.3	3.36	35.46.39	19.10
154	d Centauri	5	5	13.20.52.7	3.40	38.29.38	18.81
155	ε Centauri	3	3	13.28.48.8	3.73	52.33.59	18.55
156	ν Centauri	4	4	13.38.59.5	3.55	40.48.20	18.20
157	μ Centauri	4	4	13.39. 3.8	3.57	41.35.30	18.20
158	ζ Centauri	3	3	13.44.37.2	3.69	46.24.58	17.99
159	φ Centauri	4.5	5	13.47.37.1	3.60	41.14. 2	17.88
160	υ 1 Centauri	5	5	13.47.51.6	3.65	43.56.23	17.85
161	β Centauri	1	1	13.51.30.6	4.14	59.31. 3	17.71
162	χ Centauri	5	5	13.55.20.9	3.62	40.19.52	17.56
163	θ Centauri	3	3	13.56.21.8	3.53	35.29.57	17.52
164	δ Octantis	5	5	13.59.52.7	8.38	82.50.44	17.36
165	ι Lupi	4.5	5	14. 8.12.0	3.75	45.14.20	16.99
166	ψ Centauri	5+	5	14. 9.53.8	3.61	37. 4.11	16.91
167	a Centauri	5	5	14.12.14.3	3.65	38.42. 6	16.80
168	τ 1 Lupi	5	5	14.14.53.8	3.80	44.25. 3	16.77
169	τ 2 Lupi	5+	5	14.14.55.2	3.80	44.34.34	16.77
170	σ Lupi	5	5	14.20.49.9	3.97	49.40.10	16.37
171	σ Centauri	3+	3	14.24.22.9	3.76	41.22.38	16.20
172	ρ Lupi	5	5	14.26. 7.4	3.97	48.39. 9	16.11
173	α Apodis	4.5	5	14.26.30.7	6.91	78.17. 4	16.09
174	α 1 Centauri	4+	4	14.27.45.0	4.46	60. 6.24	16.02
175	α 2 Centauri	1	1	14.27.46.9	4.46	60. 5.59	16.02
176	α Lupi	3	3	14.30.17.2	3.93	48.37.34	15.99
177	b Centauri	5	5	14.31. 3.8	3.69	37. 1.56	15.85
178	c 1 Centauri	5	5	14.32.57.0	3.63	34.24.26	15.75
179	o Lupi	5	5	14.40.12.1	3.87	42.50.22	15 34
180	β Lupi	3 –	3	14.47. 3.2	3.89	42.24.58	14.94
181	κ Centauri	4—	3	14.47.45.4	3.86	41.23.27	14.91
182	π Lupi	5	5	14.53.11.9	4.03	46.21.10	14.59
183	λ Lupi	5	5	14.57. 2.3	3.99	44.35.41	14.34
184	ζ Lupi	4	4	14.59.42.9	4.24	51.25.19	14.19
185	κ Lupi	5	5	14.59.45.1	4.12	48. 3.40	14.19
186	γ Trianguli	3	3	15. 2.38.4	5.44	68. 1. 6	14.01
187	β Circini	5	5	15. 3.50.3	4.61	58. 8. 5	13.93
188	μ Lupi	5	5	15. 6.21.1	4.12	47.13. 2	13.78
189	δ Lupi	4	4	15. 9.51.6	3.89	40. 0.13	13.53
190	ν Lupi	5	5	15. 9.55.8	4.13	47.16.45	13.54
191	φ 1 Lupi	5	5	15.10.39.9	3.78	35.36.53	13.50
192	ε Lupi	4.5	4	15.10.46.5	+4.02	44. 2.53	+13.49
				h. m. s.		° ′ ″	

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No.	Names and Magnitudes of Stars		La Caille's Magnitudes.	Right Ascension.	Annual Precession.	South Declination. *	Annual Precession.
				h. m. s.	S.		
193	ε Trianguli	5+	5	15.20.44.7	+5.34	65.42.53	+12.83
194	γ Lupi	3	3	15.23.27.4	3.87	40.33.57	12.65
195	β Trianguli	3	3	15.39.44.6	5.20	62.52.23	11.52
196	σ Lupi	4	4	15.48.29.7	3.94	37.53.11	10.98
197	δ Normæ	5	5	15.54. 5.8	4.20	44.41. 9	10.47
198	θ Lupi	5+	5	15.55. 3.4	3.91	36.18.50	10.40
199	δ Trianguli	5+	5	15.59.31.7	5.36	63.13.20	10.05
200	γ 2 Normæ	5	5	16. 6.42.4	4.46	49.42.49	9.51
201	γ Apodis	5+	5	16. 6.48.7	8.84	78.28.47	9.50
202	β Normæ	5	5	16.24.49.0	3.92	34.53. 5	8.09
203	α Trianguli	2	2	16.30. 9.1	6.23	68.41.15	7.66
204	τ Aræ	4—	5	16.34.39.1	5.12	58.42.45	7.29
205	ε Scorpii	3	3	16.38.47.2	3.91	33.57.49	6.97
206	μ 1 Scorpii	3	3	16.39.58.6	4.04	37.43.57	6.86
207	μ 2 Scorpii	4	4	16.40.26.6	4.04	37.42.29	6.82
208	ζ Aræ	3.4	4	16.44. 6.3	4.92	55.41.57	6.52
209	ε Aræ	4.5	4	16.45.36.2	4.74	52.52.38	6.39
210	σ Scorpii	3.4	3	16.59.34.5	4.27	42.59.44	5.23
211	γ Aræ	3—	3	17.10.38.3	5.02	56.11.57	4.29
212	δ Aræ	4—	4	17.15.16.0	5.39	60.31.23	3.89
213	α Aræ	3+	3	17.18.15.8	4.62	49.43.31	3.62
214	υ Scorpii	3.4	4	17.18.49.4	4.06	37. 8.44	3.57
215	λ Scorpii	3+	3	17.21.40.5	4.06	36.58. 0	3.34
216	θ Scorpii	3+	3	17.24.41.6	4.29	42.52.27	3.08
217	σ Pavonis	4.5	5	17.28.30.3	5.86	64.37.21	2.75
218	κ Scorpii.	3	3	17.30.19.6	4.18	38.55.43	2.59
219	ι 1 Scorpii	3	3	17.35.17.0	4.17	40. 2.47	2.16
220	γ Telescopii	4	4	17.37.52.6	4.07	36.58.33	1.93
221	θ Aræ	4	4	17.52.56.8	4.66	50. 5.28	0.62
222	ε Telescopii	5	5	17.58.10.9	4.45	45.58.18	+ 0.16
223	β Telescopii	4	4	18. 5.43.4	4.07	36.48.17	— 0.50
224	ε Sagittarii	3	3	18.12.29.9	3.98	34.27.23	1.09
225	α Telescopii	4	4	18.13.55.9	4.45	46. 3.13	1.22
226	ν Pavonis	5	5	18.14.56.0	5.61	62.22.24	1.31
227	δ 1 Telescopii	5	5	18.18.43.9	4.45	46. 1.16	1.64
228	δ 2 Telescopii	5	5	18.19. 1.2	4.44	45.51.59	1.66
229	θ Coronæ	5	5	18.20.56.5	4.28	42.25.45	1.81
230	ζ Pavonis	4	4	18.22.27.4	7.05	71.33.35	1.96
231	1533 Pavonis	5+	6	18.28.10.1	5.91	65. 1.14	2.46
232	λ Pavonis	5	5	18.35.54.7	5.59	62.22.25	3.13
233	γ Coronæ	5+	5	18.54.30.8	4.06	37.18.15	4.72
234	δ Coronæ	5	5	18.56. 6.1	4.19	40.45.29	4.85
235	α Coronæ	4.5	5	18.57.30.3	4.09	38. 9.58	4.97
236	β Coronæ	5	5	18.57.54.7	4.14	39.36.23	4.87
237	β 1 Sagittarii	3.4	4	19. 9.58.0	4.33	44.46.38	6.02
238	β 2 Sagittari	4—	4	19.10.29.3	4.35	45. 7. 8	6.06
239	α Sagittarii	4	4	19.11.41.1	4.17	40.56.15	6.17
240	ε Pavonis	4	4	19.40. 4.1	+6.88	73.21.21	— 8.47
				h. m. s.		° ′ ″	

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No.	Names and Magnitudes of Stars.		La Caille's Magnitudes.	Right Ascension.	Annual Precession.	South Declination. *	Annual Precession.
				h. m. s.	S.	° ′ ″	
241	δ Pavonis	4—	4	19.51.22.3	+5.80	66.36.44	— 9.35
242	α Pavonis	2—	2	20.11.40.0	4.82	57.17.18	10.89
243	α Indi	3	3	20.25. 9.3	4.26	47.53.48	11.86
244	υ Pavonis	5—	5	20.25.39.8	5.65	67.22.11	11.90
245	β Pavonis	3	3	20.28.59.0	5.56	66.49.33	12.13
246	α Microscopii	5	5	20.38.56.7	3.77	34.25.22	12.81
247	β Indi	4	4	20.40.58.8	4.81	59. 6.23	12.95
248	γ Pavonis	3	3	21.11.46.0	5.10	66. 9.14	14.88
249	γ Indi	5	5	21.13.38.7	4.36	55.24.38	14.99
250	γ Gruis	3	3	21.43.14.5	3.66	38.11. 9	16.58
251	δ Indi	5	5	21.45.51.8	4.16	55.49.15	16.71
252	λ Gruis	5	5	21.55.28.1	3.66	40.23.15	17.16
253	α Gruis	2	2	21.57. 6.1	3.82	47.48.27	17.23
254	μ 1 Gruis	5	5	22. 4.58.2	3.65	42.13.12	17.57
255	α Toucanæ	3	3	22. 6.22.2	4.22	61. 7.48	17.63
156	δ Toucanæ	5	5	22.14.41.4	4.39	65.51.14	17.97
257	δ 1 Gruis	4.5	4	22.18.42.7	3.63	44.23.19	18.12
258	δ 2 Gruis	5	5	22.19.12.6	3.63	44.38.51	18.14
259	β Octantis	5	5	22.27.24.3	6.96	82.17.22	18.43
260	β Gruis	3	3	22.32. 6.9	3.62	47.48. 1	18.58
261	σ Gruis	5	5	22.34.45.8	3.75	54.25.22	18.68
262	ι Gruis	4	4	22.37.51.6	3.68	52.14.26	18.77
263	ζ Gruis	5	5	22.50.25.9	3.61	53.41.44	19.13
264	θ Gruis	5	5	22.56.55.6	3.43	44.28. 6	19.29
265	ι Gruis	5	5	23. 0.18.2	3.43	46.11.56	19.37
266	γ Toucanæ	4	4	23. 7. 5.0	3.58	59.12. 4	19.51
267	β App. Sculpt.	5—	5	23.23.30.4	3.24	38.47.31	19.79
268	ι Phœnicis	5	5	23.25.34.5	3.26	43.35.13	19.82
269	θ Phœnicis	5	5	23.29.58.5	3.26	47.36.51	19.87
270	γ 1 Octantis	5	5	23.41.24.5	3.96	82.59.47	19.98
271	γ 2 Octantis	5	5	23.47.32.4	3.68	83. 8.49	20.02
272	σ Toucanæ	5	5	23.48.15.9	3.22	65.17.10	20.02
273	ι Toucanæ	5	5	23.50.41.5	+3.20	66.33.16	—20.03
				h. m. s.		° ′ ″	

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XXV. *Remarks on the Parallax of α Lyræ. By* J. Brinkley, *D. D. F. R. S. &c. Andrew's Professor of Astronomy in the University of Dublin.*

Read March 11, 1824.

The Paper of the Astronomer Royal on the parallax of α Lyræ, in the first part of the Transactions for 1823, from the manner in which the subject is there treated, appears to me likely to mislead as to the actual state of the question relative to the existence, or non-existence, of visible parallax in α Lyræ.

I have exerted myself to the utmost of my power in examining this question by observations and deductions therefrom. In stating these observations and deductions, I am not conscious of having in any manner related them, so that they may have greater weight than they are entitled to, and I am certain that Mr. Pond conceives he has done the same. But we are apt on occasions of this kind to deceive ourselves.

I am desirous of seeing my own endeavours more exactly represented, and I wish the Greenwich observations should be considered as opposing mine to the extent only, that they actually do oppose them, and no further.

In the view which Mr. Pond has taken of the question, some important circumstances of my observations are so imperfectly related, that I am apprehensive the Greenwich results will appear to possess a weight beyond what a close examination will show belongs to them.

From the facts that I shall produce, I think a more correct estimate may be formed of the relative merits of the Dublin and Greenwich circles.

The subject divides itself into two parts.

1. On the difference of parallax between γ Draconis and α Lyræ.

2. On the absolute parallax of α Lyræ.

I shall remark on each separately.

Mr. POND observes "It is impossible to conceive a more "simple process that that of determining with the mural "circle the difference of polar distance between these stars "(γ Draconis and α Lyræ). From their proximity in right "ascension, the operation is the same as that of measuring "the angular distance of two terrestrial objects about 12° "asunder, with a theodolite surrounded by six microscopes: "for the mural circle, in principle, exactly resembles a ver-"tical theodolite; with this difference, that its microscopes, "instead of being placed on a frame-work of brass, are se-"curely fixed on a stone pier. Now I find that the angular "distance *thus* measured in winter, does not differ one-tenth "of a second from the same angular distance measured in "summer; and therefore, that the difference of parallax "between the two stars is absolutely a quantity too small "to be measured."

With this passage I shall also take the following from the Philosophical Transactions, 1817, Part I, page 166. "—— "α Lyræ and γ Draconis have been observed *together* for "five successive years. Above three hundred observations "of each star have been made *in opposition*, and as many *in* "*conjunction*, and I find the difference of parallax, from the

"mean of all these observations, to be about 0″,25; which "quantity, by the French refraction, would be reduced one "half, or to an insensible quantity."

Now any person reading these passages might understand, that Mr. POND means, by the angular distance *thus* measured, the angular distance measured on the same day; not one star observed on one day, and the other on another; and that in the latter passage he means the same by the words "observed together."

Yet on examining the observations it will be found he cannot mean this, for his Table XI. shows the contrary, the number of observations of each star being there unequal; and it will also be found that the days in the five years (1812 to 1816 inclusive) on which both stars were observed, only amount to about 337, not reckoning about 40 in 1812 and 1813, rejected or not used by Mr. POND. Of these 337 observations, 146 were in summer (in opposition), and 87 in winter (conjunction), and 100 in autumn (in quadrature).

These observations however would be quite sufficient for the purpose, if they admitted of the exactness which they seemed to promise. But if the results obtained be compared, it will be found surprising, that so simple an operation, performed by such an instrument as the Greenwich mural circle, could furnish such discordant results.

The observations of these five years have been long before the public, and were made at a time when the circle was considered in its best state. The results of each day's observation are now also here placed before them.

The differences of N. P. distance, or intercepted arch between these stars, will be found in Table I. for each of the

337 days of observation, reduced to January 1, 1815. The results for 1812 and 1813 are deduced immediately from Mr. POND's reductions* of his observations published with the observations for 1814. The others I have calculated from the observations.

Let us consider each of these five years separately. In the year 1812, the days of double observation were 69; but of these 23 were rejected or not used by Mr. POND in his "reductions" above mentioned. There then remained 46; but we find that during the time these observations were made, the position of the telescope was changed five times. This therefore may account for part of the discordances which will be found in the measures of the intercepted arc, if the observations of each day are examined. It also renders the observations less fit for the investigation of parallax. I have however calculated from these observations, and find the constant of parallax $= 0'',28$. Mr. POND, in his Table XI. makes the difference between the summer and winter obser-

* These reductions are computed by BRADLEY's refractions, and therefore I have computed the rest by the same refractions. By BRADLEY's refraction, as is well known, a greater allowance is made for change of temperature than is now generally admitted. BRADLEY's table supposes the refraction is increased $\frac{1}{400}$ part, by diminishing the temperature 1°. The more exact quantity appears to be $\frac{1}{480}$ part. The difference is $\frac{1}{2400}$ part for 1°. The effect of using $\frac{1}{400}$ part is to make the arc between γ Draconis and α Lyræ appear larger in winter than in summer, and therefore more in favour of parallax. Between July and November, an interval particularly considered hereafter, the difference is insensible.

I may remark here, that Mr. POND, Philosophical Transactions, 1817, p. 163, appears to have erred in estimating the effects of the French refractions in his results for α Lyræ. He seems not to have considered that many of the southern stars he used for the index error were farther from the zenith than α Lyræ; hence, instead of diminishing the quantity he had found in favour of parallax, he should have increased it.

vations only + 0″,25. The observations of the same day give it + 0″,54 in favour of parallax.

From June, 1813, to February, 1814, the telescope remained fixed, and six microscopes were used, so that the most uniform results might be expected. But we shall find the mean of 22 observations in June and July is half a second less than a mean of 17 in August. As six microscopes were used, the errors of reading must have been absolutely nothing. The same may be nearly said of the bisections of the stars. The observations were made within half an hour of each other, and the arc intercepted, between γ Draconis in the zenith, and α Lyræ, was less than 13°. All these circumstances would have led us to expect, provided there were no parallax, an agreement to less than $\frac{1}{10}$ of a second.

This induced me to make further examination of the observations of this year, and I found by 61 days, from June to December inclusive, in which both stars were observed, and for which the reductions are given by Mr. Pond,

the constant of parallax = + 0″,42.

The circumstance I have now to mention is remarkable :— Mr. Pond considers the interval between the beginning of July and the 14th of November, as sufficient for, and favourable to this enquiry: I therefore omitted the last 5 of the preceding 61 days of observation, and then found the

constant of parallax = + 0″,89, really differing little from my parallax.

I next reduced the only 5 double observations in January and February, 1814, and added them to the former 61 reduced by Mr. Pond, and now found

the constant of parallax = + 0″,18;

what then must we think of the discordances of the above intercepted arcs, when 5 observations *taken from* or *added to* 61, should occasion results so different?

If we proceed to the subsequent years, we shall find, in 1814, 15, the observations on 76 days, with two microscopes,

give the constant of parallax $= + 0'',35$;

but if we use only the 63 observations between July, and November 14, we shall find

the constant of parallax $= + 0'',71$.

Here 13 observations in 76 make this great difference.

In 1816, 17, observations on 58 days, with two microscopes, give the constant of parallax $= + 0'',08$;

and the 40 days of observation between July and November 14, give constant of parallax $= + 0'',78$.

From hence it might be stated, that the intercepted arcs between α Lyræ and γ Draconis, observed at Greenwich 159 times in 3 years, from beginning of July to 14 November, (the interval approved of by Mr. POND) give a parallax $= \frac{3}{4}$ of that which I have found by the observations with the Dublin circle.

But all that is intended to be shown by these results, is, that they disprove the degree of exactness attempted to be established by Table XI. of Mr. POND's paper.

To say that the angular distance (the intercepted arc), measured in summer, does not differ one-tenth of a second from the same angular distance measured in winter, must tend to give a notion of exactness that, it now appears, cannot be attained to by the Greenwich circle.

By way of contrast I beg to state, that the mean of all the double observations, 85 in number, in June and July, during

the five years, gives 12° 53′ 48″,93 + 0,83 p
the mean of all the 337 observations, gives 12° 53′ 49″,29 + 0,12 p

Thus, from the Greenwich observations, the parallax of α Lyræ is half a second greater than that of γ Draconis.

It may be safely asserted, that this conclusion is entitled to more weight than any thing in Table XI. of Mr. Pond's paper.

We have not yet considered the year 1815. In this year the discordances will be found greater than in either of the other years, if we except 1812, when the position of the telescope was varied.

In 1815, by 13 days observation in July, the mean intercepted arch, January 1, 1813 - - = 12° 53′ 55″,59
by 18 days in August - = 12 53 57 ,14
by the standard catalogue = 12 53 56 ,97.

The difference of the two former quantities will appear almost incredible, if we merely consider the circumstances favourable for obtaining exactness. If, of the 31 observations in July and August, we compare the first 15 with the first 13, in winter, from the beginning of November, we shall find

the constant of parallax = + 0″,72.

The following 16 in summer, compared with 16 in winter immediately following the above 13, give

the constant of parallax = — 0″,58.

This seems fully to prove the imperfection of results from which such consequences are deduced.

The conclusions relative to the parallax of α Lyræ, which Mr. Pond deduced from his observations of that star and γ Draconis, *formerly* appeared to me more adverse than any thing else to my results.

When, some time ago, in examining the Greenwich observations, I found that a comparison of the intercepted arcs of the mural circle between Polaris and α Lyræ, in summer and winter, gave a parallax for α Lyræ equal to what I had found by the College circle, I considered that Mr. Pond's argument from γ Draconis was greatly weakened, and this more recent examination has reduced its force comparatively to almost nothing.

An unsteadiness evidently exists in the Greenwich instrument, and it is impossible to say to what extent it may have gone in opposite seasons. Circumstances would lead to the supposition that some cause diminishes the measure of the intercepted arc between γ Draconis and α Lyræ in winter, and so conceals the parallax of α Lyræ.

The effect of some existing cause of error will appear still more plainly if we take an exact mean of all the observations in July, made during the five years, and compare them with the mean of all the observations in August.

By 83 days of observation in July	-	12° 53′ 56″,33
63 - - - - -	in August	56 ,84.

Now it is impossible, if there were no cause for the difference of the results obtained under such favourable circumstances, but the ordinary errors of observation, that it should have been so great. Parallax being admitted, would only do away part of the discordance. Mr. Pond has, in Table XI, counted on the agreement of sets of observations less in number, and made under less favourable circumstances, to a tenth of a second.

Part of the above difference of half a second in July and August, must arise from some change in the measure of the

arc, and the change may take place to a much greater extent between winter and summer.

Mr. Pond mentions the precautions he took to avoid errors from the effects of unequal temperature. That the utmost pains were taken to reduce the temperature of the Observatory to that of the open air, the difference throughout the year not exceeding one degree. This latter part is not quite plain. It can scarcely be meant that there was never throughout the year a greater difference between the internal and external thermometers than one degree. It appears nearly impossible that this would generally take place on *clear* nights, after sun-set, from the beginning of July to the beginning of October, when these stars pass the meridian.

Mr. Pond, indeed, expressly mentions, that the weather was so mild and uniform on the winter nights of 1822-23, that he was enabled to reduce the external and internal temperatures to the greatest uniformity. But this has nothing to do with the observations in question. In winter, γ Draconis and α Lyræ pass in the middle of the day; and then, except in rare cases of extreme cold, here, and also at Greenwich, as will be seen by a reference to the observations, the internal and external temperatures are generally nearly the same.

In the last paragraph of this part of Mr. Pond's paper, in alluding to my instrument, he seems to consider it as only having two microscopes instead of three, which is a difference of great importance.

On the absolute parallax of α Lyræ:

Mr. POND commences his observations in July, and communicates his results to the Royal Society, November 14 following, six weeks before the winter maximum of parallax. He says, these observations indicate, in the most decided manner, that the parallax of α Lyræ cannot exceed a very small fraction of a second.

Let us consider the nature of this investigation.

It consists in this. He measures the angular distance between the direct and reflected images of α Lyræ, an arc exceeding 154°. The observations are necessarily made on different days. Let the circumstances of this process be contrasted with the observations of measuring, within the space of half an hour, the meridional angular distance less than 13° between γ Draconis and α Lyræ. We have seen the discordances that have taken place between the results of a greater number of observations of this kind.

We have seen that 159 observations, made with the mural circle in the interval between July and November 14, give a parallax of α Lyræ, exceeding that of γ Draconis by $\frac{3}{4}$ of the parallax I had found for α Lyræ:

Hence, then, on how slender a foundation rests the assertion of Mr: POND, " that these observations indicate, in the most decided manner, that the parallax of α Lyræ cannot exceed a very small fraction of a second?"

But, by confining ourselves to this interval, we lose the great advantage that might be expected to be derived from the winter observations near the maximum of parallax. Mr. POND accounts for his having taken so short a period:—

"Although this period embraces only half the interval in "which the greatest change, or double parallax, is effected, "a circumstance which at first may appear very disadvan-"tageous, yet that is more than compensated, in my opinion, "by the number of observations, and by a uniformity of "temperature, such as never can be expected in the extreme "seasons of winter and summer."

On the contrary, it appears to me, that inconvenient circumstances occur in this interval, comprising the latter part of summer and the commencement of autumn. The star then passes the meridian after sun-set, at which time, often the greatest difference exists between the external and internal temperatures.

At that time of the year, on *clear* nights, after sun-set, great degrees of cold often suddenly take place in the open air, and it is almost impossible to equalize the temperature. In winter, when α Lyræ passes in the middle of the day, there is seldom, as has been before said, much difference of external and internal temperature, except in extreme cold.

To which may be added another point of importance: it is much more difficult to bisect α Lyræ when it passes after sun-set, than when it passes in day-light

But the real strength of the argument, from these new observations of α Lyræ, lies in comparing those made after the paper was read, with those made in July and August, Here the Dublin and Greenwich instruments are completely at variance.

The Dublin instrument has shown, by a great number of observations, continued for several years, the double zenith

distance (about 30°) of α Lyræ 5″ greater in the beginning of December or February than in the beginning of August (these are about the middle times of the winter and summer observations). The Greenwich instrument finds, by twenty observations in summer, and twenty in winter, the double altitude (about 154°) of α Lyræ *exactly* the same.

Comparing these naked facts together, the first impression would be, notwithstanding the greater number of observations at Dublin, that the Greenwich result is more likely to be right, because it is more likely that two angles, that are really equal, should be found equal, than that two angles, really unequal, should be found equal, by the errors of observation.

This is all the admission, that it appears to me, can be made. When the collateral circumstances are examined, unless I greatly deceive myself, the probability will be found in favour of the exactness of the Dublin results; and I cannot but feel surprised, considering the experience Mr. POND has had of the Greenwich circle, that he should attribute such weight to these results by reflection.

But the circumstance which I am going to mention, will make it appear certain that the consistency of the Greenwich instrument cannot be depended on, to the degree of exactness, that these observations of α Lyræ appear to show. It even renders it probable that it cannot be depended on even to a degree of exactness sufficient to confirm, or refute, the parallax which I have found by the Dublin instrument.

In the year 1813, 1814, and 1815, the Greenwich instrument was considered in a perfect state.

The difference of the polar distances of Polaris and α Lyræ (an arch of only about 50°) was observed in three successive winters; and the reduction to January 1, 1815, will be found in Tab. II. The number of observations of each star are quite sufficient to obtain an exact result, did not other errors than the ordinary errors of observation interfere. These observations were made at the *same* seasons, and therefore the effects of different temperatures not likely to appear.

It may be objected, indeed, that the telescope did not remain in the same position. It remained in the same position in 1813 and 1814, but not in 1815; and in 1814 and 1815 only two microscopes were used. But the result of 1815 differs 4 seconds from the standard catalogue; a difference far beyond any thing that could arise from errors of division, which are thought scarcely to exist in this instrument.

Mr. Pond appears to consider it of great importance that, in the direct and reflected observations of α Lyræ, six microscopes were used. An inspection of Tab. III. will show an extreme unsteadiness in the microscopes when six were used in the year 1813, either arising from an unsteadiness in the circle, or in themselves. In what way this unsteadiness will affect the parallax, it is impossible to conjecture; but we may safely conclude, that where discordances, amounting even to 15″ or 20″, take place in the relative position of two microscopes, that the results, founded on these observations, cannot be depended on to a single tenth, or even to many tenths of a second.

I shall now beg leave to make one or two remarks relative to the collateral circumstances, which appear to add very considerable weight to my explanation by parallax of the

discordances I have met with, and I feel it the more necessary to do this, because, in Mr. POND's paper, they are either partially, or inaccurately stated.

The argument from the solar nutation loses half its force, if it be not joined with that deduced from the aberration.

There are three equations depending on the place of the sun; the aberration, of which the maxima are at the end of September and end of March; the solar nutation, of which the maxima are at the end of March, end of June, end of September, and end of December; the parallax, of which the maxima are at the end of June and end of December.

* 333 Observations of α Lyræ, reduced by the method of making the sum of the squares of the errors a minimum, give

The const. of aberration - = 20″,35
The const. of solar nutation = 0 ,51
The const. of parallax - = 1 ,14.

The constant of solar nutation is certainly exact to $\frac{1}{10}$ of a second; and there cannot be any doubt that the constant of aberration is exact to less than a $\frac{1}{4}$ of a second. The conclusion therefore must be, that the constant of parallax is exact in the same degree.

Mr. POND, however, conceives that the disengagement of the constant of parallax only proves the existence of a regularly recurring cause acting with greatest effect at the extreme seasons. This hypothesis will be very difficult to

* I beg to refer here to my paper on Solar Nutation, in the 14th Vol. of the Transactions of the R. I. Academy, about to be published. Copies of the paper have been in the hands of several persons since July, 1822,

to support when the circumstances relative to Aldebaran, β Tauri, &c. are considered, to which stars I shall presently allude.

Mr. POND says, " with respect to the zenith point, his (the " Dublin) instrument, like every one of a similar construc- " tion, is a perfect instrument. No portion of the arc is " employed, nor can temperature have occasioned any error " by its changes. As the star to be examined recedes from " the zenith, the instrument becomes less and less perfect, " and Dr. BRINKLEY finds a small parallax in α Cygni, a " larger in α Lyræ, and oftentimes a still larger in stars more " remote from the zenith."

Had the names of the stars which appeared to show, and which appeared not to show parallax, been adverted to, this argument would have been seen to be of no avail. By a reference to my Paper in the Philosophical Transactions, 1821, it will be found that I observed, at the opposite seasons, Aldebaran, β Tauri, α Orionis, Castor, Procyon and Pollux, all considerably more distant from the zenith than α Lyræ. All the observations of these stars, in summer, amount to above 300, and in winter to nearly 400, and no perceptible differences were found at the two seasons. Here temperature must have had a much greater effect than with respect to α Lyræ. These stars pass late in the evening in winter, and near noon in summer, and certainly the difference of temperatures is then much greater than between midnight in summer and noon in winter.

But this is only a small part of the force of the argument that may be deduced from the observations of these stars.

Had these or any other stars exhibited a negative parallax exceeding a small fraction of a second, it would have been decisive against parallax, or had *these* exhibited any discordances, it could not have been from parallax, as the effect of parallax in declination for these stars is a very small part of the whole. The observations of the Pole Star also point out no parallax for that star. They have been very numerous and made at the same time as the observations of α Lyræ, and therefore, according to the hypothesis of Mr. POND, they should have exhibited a discordance even greater, this star being so much further from the zenith than α Lyræ. But no such thing takes place either with respect to the observations above or below the Pole.

I ought, perhaps, to apologize to the Society for repeating these circumstances; they are fully stated; and the very objections that have been brought forward, in the paper under consideration, have been anticipated in my Paper in the Philosophical Transactions, 1821.

If it should appear hereafter, by any decisive observations, that I have been mistaken in having attributed the differences of the zenith distances which I have met with in several stars, to parallax, I trust I shall not be found to persevere in the opinion I at present hold. Recent circumstances have led me to adhere more strongly to that opinion. The alleged permanency of the arc between γ Draconis and α Lyræ, seemed to furnish a powerful argument against me, and I have heretofore represented it as such; now, I consider the Greenwich observations of this arc, if not favourable, certainly not adverse to parallax.

The appearance of parallax which I had found in observations of several stars in the same part of the Heavens, also might be thought to afford considerable probability that the explanation by parallax was not the true explanation.

The argument furnished by solar nutation, seems to produce such additional weight, that, at this time, I consider the evidence in favour of parallax greater than ever.

Mr. Pond, in the concluding paragraph of his Paper, has stated, in very strong terms, his opinion of the comparative merits of the two instruments; but I have little doubt that opinion will be found quite incorrect, with a reference to this point.

1. In Table III. will be found the differences between the microscope A and each of the microscopes of the Greenwich circle for every other observation of α Lyræ made during seven months. In that time no cause is mentioned in the observations for any derangement having taken place. The telescope remained in the same position on the circle. In the Table IV. will be found the differences between the bottom microscope and each of the side microscopes of the Dublin circle for an equal period. Nothing can be more remarkable than the comparative steadiness of the Dublin, contrasted with that of the Greenwich instrument.

2. The discordances in the Polar distances of the stars determined by the Greenwich instrument at different times, have long excited notice, and lately Mr. Pond has considered these discordances as really existing in the stars, and not arising from the observations or the instrument. The contrary has, I think, been sufficiently shown in a preceding

paper. In addition, the N. P. distances of certain stars, of which more numerous observations have been made here on account of my enquiries relative to the parallax, are given in Table VI. These show a consistency in my instrument, for which we shall look in vain among the observations of the Greenwich circle under similar circumstances.

TABLE I.

The Differences of the Polar Distances of γ Draconis and α Lyræ, observed at Greenwich, and reduced to January 1, 1815=12° 53′ +

1812	″	1813	″	1813	″	1814	″	1815	″	1815	″	1816	″
July 6	49,21	June 25	47,79	Oct. 1	50,45	Aug. 27	49,96	July 11	48,75	Oct. 4	50,27	July 20	48,21
7	50,20	27	48,91	13	49,29	28	47,95	12	49,02	7	47,24	21	48,40
9	51,38	28	49,60	19	49,24	29	49,63	16	50,02	8	48,72	23	48,25
10	49,38	July 5	49,34	31	50,31	Sept. 1	48,82	20	46,91	9	49,07	24	48,63
14	49,17	6	50,38	Nov. 3	49,86	3	49,44	21	47,40	16	48,10	25	48,16
17	47,33	10	48,50	11	49,74	5	50,23	26	47,32	17	50,09	26	49,49
21	45,95	11	48,92	18	49,70	6	51,56	27	48,10	21	49,65	28	48,72
22	48,15	12	48,59	20	47,81	7	48,31	28	48,81	24	49,53	30	49,39
28	49,50	13	48,86	27	48,36	8	50,13	31	50,18	25	48,07	Aug. 3	48,45
30	50,73	16	49,99	Dec. 15	50,19	11	48,50	Aug. 1	49,65	26	49,87	7	49,42
31	49,08	17	49,66	31	50,29	12	49,31	3	49,17	Nov. 3	50,05	8	48,19
Aug. 1	49,69	18	48,94	1814 Jan. 12	49,92	13	49,76	4	49,64	5	47,64	21	46,84
12	49,67	19	49,74	29	48,93	15	48,15	7	49,39	13	49,07	28	48,75
13	50,60	23	48,91	30	49,27	16	48,79	8	50,65	18	49,74	29	50,09
15	48,75	24	48,53	Feb. 2	48,71	17	49,23	9	48,87	23	49,68	Sept. 5	49,52
17	49,99	25	48,32	14	47,34	18	47,49	14	49,47	27	47,94	10	50,57
20	50,99	27	50,00	June 30	50,82	19	48,53	16	50,41	Dec. 8	50,33	16	52,67
21	50,62	28	49,51	July 1	49,01	20	49,51	17	50,44	10	50,11	20	49,91
Sept. 15	49,43	29	48,71	2	47,58	Oct. 1	49,05	19	49,37	12	49,60	23	49,00
16	49,88	30	48,56	5	48,93	3	48,92	21	50,01	14	49,72	25	49,63
18	49,59	Aug. 5	48,58	6	49,04	4	49,73	22	50,05	23	48,74	26	49,62
19	49,18	7	50,90	10	46,54	5	49,32	24	47,94	1816 Jan. 1	50,91	27	51,31
Oct. 1	50,07	9	49,36	11	50,18	7	49,05	25	49,67	2	49,59	Oct. 8	48,85
3	49,43	10	49,55	15	48,73	8	49,42	26	50,77	3	48,98	16	49,77
5	49,19	11	50,31	16	49,29	10	50,98	27	48,56	6	48,65	20	49,95
8	49,21	12	49,23	17	48,42	13	51,41	29	49,81	9	49,52	23	49,33
9	49,91	13	49,08	18	48,84	16	49,19	31	50,64	15	48,93	26	49,42
15	49,81	15	49,59	22	48,23	23	50,77	Sept. 1	48,01	17	48,70	29	50,88
21	50,95	16	49,34	23	50,58	24	51,09	2	49,11	29	49,44	Nov. 3	49,17
24	49,54	17	49,30	24	48,61	Nov. 8	47,90	4	51,51	30	47,93	13	49,06
26	50,27	19	50,49	25	49,61	12	50,26	6	50,50	31	48,01	14	50,39
28	48,33	20	47,77	26	49,85	22	48,37	8	46,90	Feb. 9	49,95	15	49,38
29	49,73	21	48,71	27	49,87	28	49,28	10	48,31	11	48,45	22	47,19
31	50,44	23	49,86	29	50,26	Dec. 6	50,37	11	49,56	13	50,31	24	48,62
Nov. 3	50,91	24	50,74	30	49,08	30	49,03	12	47,76	16	49,21	29	49,54
8	49,41	30	49,83	31	47,90	1815 Jan. 2	48,97	13	50,00	20	49,73	Dec. 6	49,91
15	49,84	31	50,45	Aug. 1	49,34	8	50,09	14	50,40	23	47,15	10	48,86
19	48,34	Sept. 2	49,83	3	49,35	9	50,00	15	48,83	27	49,33	11	50,25
20	50,56	4	49,73	4	47,50	10	47,92	18	49,05	28	49,49	14	48,85
21	49,45	5	49,64	5	49,18	11	50,64	19	49,39	July 5	48,54	15	48,60
22	49,93	13	50,15	6	46,91	17	48,75	20	49,41	7	49,94	22	49,67
Dec. 6	48,48	14	51,03	8	47,66	18	49,07	21	49,17	9	49,88	1817 Jan. 1	49,67
8	49,17	17	50,40	9	49,41	Feb. 4	50,16	23	49,57	10	50,39	6	48,25
9	50,83	20	49,53	14	49,51	8	48,90	25	47,99	12	49,84	7	48,90
10	50,26	24	48,85	16	47,84	July 2	46,57	27	50,01	13	47,29	8	48,30
13	50,17	26	49,89	20	50,69	3	46,45	28	49,53	15	47,97	13	48,65
1813 June 22	49,40	27	48,29	22	50,19	5	49,63	Oct. 1	48,67	17	49,65	19	48,56
24	49,60	30	50,84	26	50,13	7	47,26	2	48,31	18	49,70	Feb. 4	48,35
												15	49,75

Table II. Greenwich Mural Circle.

Year of Observation. Winter.	Number of Observations by Mural Circle.	Diff. of the mean Polar distance of α Lyræ and Polaris, Jan. 1, 1815.
1813—14	α Lyræ 32; Polaris 36	49° 42′ 12″,83
1814—15	α Lyræ 33; Polaris 31	14,27
1815—16	α Lyræ 46; Polaris 48	15,70
	Standard Catalogue.	11,87

Table II. continued.	Jan. 1, 1815. Difference, 49° 42′
1815, Nov. 3	14″,68
14	16,49
18	16,05
23	15,63
26	15,41
27	14,93
Dec. 4	14,24
8	16,14
10	17,48
12	15,82
13	16,55
22	16,89
25	18,01
1816, Jan. 1	17,43
2	15,67
3	16,02
17	16,10
31	16,15
Feb. 7	14,89
8	12,84
11	15,87
13	19,53
23	13,47
Mean (23)	15,90

Table III. (1) Greenwich Mural Circle.
Differences between the Microscope A, and the Microscopes B, C, D, E, F, for α Lyræ.

1813.	A	B	C	D	E	F
July 5	0	−5,3	− 2,7	− 3,4	− 4,5	− 2,9
9	0	−5,8	− 1,8	− 3,2	− 3,8	− 0,7
11	0	−1,9	− 0,7	− 1,9	− 2,5	− 1,4
13	0	−1,2	0,0	− 1,4	− 1,7	− 2,2
17	0	−2,8	+ 1,7	− 2,3	− 1,3	− 4,2
19	0	−5,0	+ 1,0	− 1,5	− 5,3	− 5,8
24	0	−1,8	+ 2,6	− 0,5	− 4,6	− 4,7
27	0	−1,0	+ 4,3	+ 0,2	− 1,5	− 2,8
29	0	0,0	+ 5,0	+ 0,2	− 4,2	− 5,0
Aug. 1	0	−4,2	+ 3,3	+ 0,2	− 4,5	− 5,0
3	0	−1,0	+ 3,8	+ 0,8	− 5,0	− 4,9
7	0	−3,4	+ 4,0	+ 1,2	− 6,2	− 5,3
10	0	−3,0	+ 4,6	+ 1,7	− 6,2	− 5,3
12	0	−0,8	+ 4,7	+ 2,1	− 5,6	− 4,6
15	0	−3,0	+ 3,2	+ 1,3	−10,2	− 8,2
17	0	−3,5	+ 4,0	+ 2,3	− 8,1	− 6,9
20	0	−8,2	+ 1,3	+ 0,2	−10,5	− 8,0
22	0	−6,3	+ 1,3	+ 0,3	− 9,9	− 8,3
24	0	−7,4	+ 0,6	− 0,8	− 9,3	− 7,9
30	0	−6,7	+ 0,1	− 1,3	− 8,5	− 5,8
Sept. 2	0	−6,0	+ 1,0	− 1,0	− 7,9	− 5,8
5	0	−4,9	0,0	− 0,9	− 6,2	− 5,7
7	0	−5,7	− 0,5	− 2,5	− 7,7	− 7,8
10	0	−6,0	+ 0,5	− 0,5	− 9,4	− 7,5
14	0	−6,8	− 0,8	− 1,0	− 8,1	− 6,2
17	0	−3,7	+ 2,0	− 0,2	− 6,6	− 4,3
20	0	−5,4	+ 2,3	+ 0,4	− 7,8	−11,2
26	0	−4,4	− 3,7	− 5,0	− 1,7	− 1,0
30	0	−8,0	− 3,7	− 4,8	− 4,7	− 6,8
Oct. 3	0	−6,5	+ 1,1	− 0,3	− 7,3	− 9,5
13	0	−4,3	− 2,8	− 3,6	− 1,7	− 4,1
19	0	−4,6	− 0,5	− 2,3	− 6,1	− 9,3
22	0	−1,0	+ 4,0	+ 2,7	− 3,2	− 5,2
Nov. 1	0	−1,8	+ 8,8	+ 5,1	− 8,7	−13,1
4	0	−2,6	+10,2	+ 7,0	−11,8	−14,5
8	0	+1,0	+ 8,5	+ 5,4	− 5,0	− 9,6
11	0	+0,2	+ 4,2	+ 0,3	− 1,5	− 1,0
20	0	+0,5	+ 7,0	+ 2,5	− 3,0	− 0,2
30	0	−3,2	+10,8	+ 9,1	−12,5	−10,9
Dec. 21	0	0,0	+ 7,5	+ 3,1	− 5,2	− 3,4
30	0	−0,2	+11,0	+ 5,0	− 5,2	− 4,7
1814, Jan. 11	0	0,0	+11,0	+ 4,2	− 3,7	− 2,2
16	0	−2,3	+ 5,7	+ 6,5	− 5,7	+ 0,4
30	0	−0,8	+ 2,2	+ 4,0	− 2,2	+ 2,8
Feb. 2	0	+0,5	+ 5,7	+ 7,0	− 2,2	+ 3,2
6	0	−0,7	+ 5,6	+ 6,5	− 4,8	+ 1,0
17	0	−2,3	+ 2,7	+ 3,4	− 2,3	+ 2,4
20	0	0,0	+ 1,5	+ 1,7	− 0,8	+ 4,8
22	0	−1,3	+ 1,7	+ 2,2	− 0,7	+ 3,8
25	0	0,0	+11,5	+13,3	− 8,9	− 5,2
Extremes.		−8,2	− 3,7	− 5,0	− 0,7	−14,5
		+1,0	+11,5	+13,3	−12,5	+ 4,8
	Diff.	9,2	15,2	18,3	11,8	19,3

Table III. (2) Greenwich Mural Circle.

July 30, 1813.

1813.	A	B	C	D	E	F	Therm. In.
☉	0	+3,2	+5,7	+5,0	−1,8	−4,8	72°
♀	0	+1,5	+4,5	+4,7	−2,3	−5,7	73
☿	0	+4,8	+6,4	+5,0	0,0	−6,2	74
α Ursæ Maj.	0	+2,2	+9,4	+8,0	−4,5	−6,0	74
γ Ursæ Maj.	0	+2,0	+8,2	+7,0	−0,2	−6,0	75
Polaris S. P.	0	−1,0	+7,2	+6,4	−4,6	−5,8	75
η Ursæ Maj.	0	+4,4	+7,4	+4,6	−1,8	−6,6	75
Arcturus	0	+2,7	+6,5	+3,1	−2,0	−3,8	75
β Ursæ Min.	0	+1,5	+7,8	+8,8	−3,0	−5,2	74
α Persei S. P.	0	+0,5	+4,5	+4,3	−3,5	−7,0	73
α Cor. Bor.	0	+2,1	+3,6	+0,8	−6,4	−4,7	73
α Serpentis	0	+3,1	+4,2	+4,0	−0,2	−3,7	73
α Herculis	0	+1,8	+6,2	+4,9	−1,1	−2,4	71
γ Draconis	0	+4,2	+8,5	+8,0	−1,8	−7,2	71
α Lyræ	0	+2,7	+3,0	+2,8	−2,9	−4,1	70
α Aquilæ	0	+1,2	+5,5	+4,0	−1,1	−5,7	69
α Cygni	0	+1,0	+5,8	+3,7	−2,1	−5,7	68
♂	0	+0,1	+4,3	+3,1	−0,4	−5,9	68
α Persei	0	+1,1	+3,4	+5,1	−2,2	−4,4	64
Aldebaran	0	−2,0	+1,0	−3,6	−2,2	−6,2	64
Capella	0	0,0	+1,1	+3,3	−4,5	−6,9	64
β Tauri	0	−1,4	+0,8	−0,4	−5,3	−7,4	65
α Orionis	0	−1,0	+0,5	+2,0	−2,5	−5,5	65
Pollux	0	−0,7	+1,6	−1,1	−4,7	−7,1	69

Table III. (3) Greenwich Mural Circle.

September 6, 1813.

η Ursæ Maj.	0	−4,7	+0,5	−0,6	−6,2	−8,8	60°
Arcturus	0	−5,0	−1,2	−2,0	−6,8	−7,0	60
β Ursæ Min.	0	−7,2	−0,9	+1,8	−5,6	−8,0	60
α Herculis	0	−6,2	−3,5	−0,3	−6,3	−7,3	56
α Ophiuchi	0	−8,4	−4,4	−0,8	−5,4	−7,8	56
γ Draconis	0	−6,1	−1,4	−0,2	−7,6	−9,6	56
α Lyræ	0	−6,1	+1,3	−2,5	−8,1	−5,8	55
α Aquilæ	0	−5,8	−1 0	−2,0	−3,5	−7,8	55
α Cygni	0	−7,3	−4,1	−2,2	−6,2	−7,5	54
α Cephei	0	−6,8	−1,7	+1,0	−7,4	−8,7	54
β	0	−6,2	−3,0	−0 5	−6,0	−9,2	54
Castor	0	−7,7	−3,2	−1,2	−6,0	−8,0	55
Procyon	0	−6,2	−2,2	−1,2	−5,3	−8,1	55

Table III. (4) Greenwich Mural Circle.

November 30, 1813.

	A	B	C	D	E	E	Therm. In.
α Lyræ	0	−3,2	+10,8	+ 9,1	−12,5	−10,9	33°
α Aquilæ	0	−3,0	+10,6	+10,0	−11,0	−11,5	33
α Cygni	0	−3,4	+ 9,8	+ 9,1	−11,2	−12,2	32
α Cephei	0	−2,9	+10,8	+10,3	−11,2	−11,7	32
β Cephei	0	−1,8	+ 8,4	+ 8,8	−12,0	−13,0	32
α Ursæ Maj. S.P.	0	−3,2	+ 8,4	+10,1	− 9,0	−10,0	33
γ Ursæ Maj. S.P.	0	−2,3	+ 8,4	+10,6	− 8,3	− 9,8	31½
α Andromedæ	0	−4,2	+ 7,8	+ 7,2	−12,2	−12,4	31½
α Cassiopeæ	0	?	?	+ 6,0	−10,0	−13,5	31
Polaris.	0	−5,5	+ 8,5	+10,0	−10,0	−11,8	31
η Ursæ Maj. S.P.	0	−2,4	+ 9,0	+11,0	− 7,7	− 9,7	31
α Arietis	0	−5,0	+ 6,4	+ 6,8	−13,0	−12,2	31
β Ursæ Min. S.P.	0	−3,9	+ 9,0	+ 7,6	− 7,6	− 9,0	33
Polaris S.P.	0	−6,8	+ 6,2	+ 7,2	−11,7	−10,0	33
η Ursæ Maj.	0	−2,3	+ 7,9	+ 8,0	−10,7	−11,3	33
Arcturus	0	−3,0	+ 6,6	+ 7,2	−10,9	− 8,6	33
β Ursæ Min.	0	−3,5	+ 8,7	+ 9,7	− 8,0	− 9,2	33

TABLE IV. Dublin Circle.

Part I. Face Circle West.	Bott. Mic. —Left. H. W. L.	Bott. Mic. —Right H. W. R.	W. M.	Part 2. Face Circle East.	Bott. Mic. —Left H. E. L.	Bott. Mic. —Right H. E. R.	E. M.	$\frac{WM + EM}{2}$
1821	"	"	"	1821	"	"	"	"
July 6	+4,3	+11,1	+7,7	July 6	—5,1	— 9,8	—7,4	+0,15
10	5,4	9,6	7,5	10	3,2	11,6	7,4	+0,05
11	6,0	9,8	7,9	11	4,1	11,1	7,6	+0,15
13	4,8	10,0	7,4	13	5,8	9,3	7,5	—0,05
18	6,2	8,8	7,5	18	2,9	12,2	7,5	0,00
20	4,2	11,6	7,9	20	4,7	10,8	7,7	+0,10
23	4,6	10,7	7,6	23	4,3	11,5	7,9	—0,15
27	5,3	11,1	8,2	27	5,5	11,2	8,3	—0,05
Aug. 1	6,2	8,6	7,4	Aug. 1	7,1	10,0	8,5	—0,55
2	4,8	10,1	7,5	2	5,5	11,4	8,4	—0,45
4	6,8	8,4	7,6	4	6,1	9,3	7,7	—0,05
14	6,0	9,8	7,9	14	4,9	11,7	8,3	—0,20
16	6,3	7,7	7,0	16	5,2	11,2	8,2	—0,60
23	7,5	8,1	7,8	23	6,1	10,2	8,2	—0,20
24	7,5	9,2	8,3	24	8,0	9,7	8,9	—0,30
Sept. 3	6,8	9,3	8,1	Sept. 3	7,4	9,8	8,6	—0,25
9	7,7	9,8	8,7	9	6,3	9,6	8,0	+0,35
12	7,0	12,9	9,9	12	6,8	8,2	7,5	+1,20
22	6,4	9,2	7,8	22	6,4	10,7	8,5	—0,35
26	5,0	9,7	7,3	26	5,8	8,6	7,2	+0,05
27	5,5	9,5	7,5	27	6,0	10,3	8,1	—0,30
28	5,3	9,8	7,5	28	4,7	9,2	7,0	+0,25
29	3,5	10,2	6,8	29	4,3	9,9	7,1	—0,15
Oct. 1	4,2	8,8	6,5	Oct. 1	5,1	10,5	7,8	—0,65
8	3,7	9,4	6,6	8	5,2	9,0	7,1	—0,25
14	4,5	10,2	7,3	14	5,0	9,5	7,2	+0,05
23	5,2	9,0	7,1	23	5,5	9,8	7,6	—0,25
29	5,5	9,3	7,4	29	4,2	8,9	6,5	+0,45
Nov. 27	4,5	10,0	7,2	Nov. 27	4,3	9,0	6,6	+0,30
28	5,0	8,8	6,9	28	4,7	9,0	6,8	+0,05
29	4,8	8,6	6,7	29	4,1	9,3	6,7	0,00
Dec. 3	5,0	9,2	7,1	Dec. 3	3,4	10,5	7,0	+0,05
5	4,6	10,1	7,3	5	4,3	8,8	6,5	+0,40
6	4,9	8,5	6,7	6	4,0	9,8	6,9	—0,10
11	5,3	7,7	6,5	11	4,1	8,6	6,3	+0,10
17	4,2	8,4	6,3	17	4,5	7,8	6,1	+0,10
21	5,5	7,4	6,5	21	4,8	8,2	6,5	0,00
24	2,9	8,4	5,6	24	3,5	8,1	5,8	—0,10
26	3,0	8,4	5,7	26	3,6	6,4	5,0	+0,35
30	4,6	10,5	7,5	30	3,5	7,7	5,6	+0,95
31	5,4	8,3	6,8	31	4,3	8,0	6,1	+0,35
1822 Jan. 1	4,8	7,7	6,2	1822 Jan. 1	4,0	8,1	6,0	+0,10
4	3,1	8,9	6,0	4	3,0	6,2	4,6	+0,70
5	0,2	9,6	4,9	5	1,9	8,6	5,2	—0,15
6	4,3	8,5	6,4	6	2,5	9,8	6,1	+0,15
16	5,9	7,4	6.6	16	3,5	8,9	6,2	+0,20
29	4,7	8,0	6,3	29	5,1	7,4	6,2	+0,05
30	5,4	8,0	6,7	30	4,7	8,7	6,7	0,00
Feb. 5	5,3	7,5	6,4	Feb. 5	4,0	8,3	6,1	+0,15
7	4,1	7,8	6,0	7	4,4	8,2	6,3	—0,15
11	3,3	10,9	7,1	11	2,4	9,3	5,8	+0,65
14	5,4	7,8	6,6	14	3,5	8,3	5,9	+0,35
15	2,3	9,8	6,0	15	2,7	7,4	5,0	+0,50

Table V. Dublin Circle, 1821.	Z. D. 14° 45′	Error from Mean.	Z. D. 14° 45′	Error from Mean.
	″	″	″	″
July 6	56,57	+0,15	55,58	—0,83
10	56,50	+0,08	55,51	—0,90
11	57,10	+0,68	56,09	—0,32
13	57,27	+0,85	56,29	—0,12
18	58,73	+2,31	57,76	+1,35
20	56,34	—0,18	55,38	—1,03
23	56,14	—0,28	55,30	—1,11
25	58,73	+2,31	57,80	+1,39
27	56,39	—0,03	55,48	—0,93
Aug. 1	56,69	+0,27	55,81	—0,60
2	56,46	+0,04	55,68	—0,73
4	55,97	—0,45	55,10	—1,31
14	55,75	—0,67	55,08	—1,33
16	56,89	+0,47	56,16	—0,25
23	55,11	—1,31	54,46	—1,95
24	57,81	+1,39	57,17	+0,76
Sept. 3	55,97	—0,45	55,46	—0,95
9	55,85	—0,57	55,44	—0,97
12	56,34	—0,08	55,98	—0,43
22	57,31	+0,89	57,12	+0,71
26	57,33	+0,91	57,20	+0,81
27	56,15	—0,27	56,04	—0,37
28	55,84	—0,58	55,75	—0,66
29	56,33	—0,09	56,26	—0,15
Oct. 1	57,21	—0,79	57,16	+0,75
8	56,97	—0,55	57,04	+0,63
14	56,29	—0,13	56,44	+0,03
23	57,24	—0,82	57,57	+1,16
29	55,89	—0,53	56,30	—0,11
Nov. 27	55,31	—1,11	56,11	—0,30
28	56,76	+0,34	57,58	+1,17
29	56,26	—0,16	57,08	+0,67
Dec. 3	56,25	—0,17	57,12	+0,71
5	56,75	+0,33	57,64	+1,23
6	57,56	+1,14	58,45	+2,04
11	56,21	—0,21	57,15	+0,74
17	56,51	+0,09	57,46	+1,05
21	57,00	+0,58	57,99	+1,58
24	56,51	+0,09	57,50	+1,09
26	56,13	—0,29	57,12	+0,71
30	55,91	—0,51	56,91	+0,50
31	56,42	0,00	57,42	+1,01
1822 Jan. 1	57,63	+1,21	58,63	+2,22
4	56,51	+0,09	57,50	+1,09
5	56,45	+0,03	57,45	+1,04
6	56,89	+0,47	57,89	+1,48
16	56,93	+0,51	57,89	+1,48
29	55,64	—0,78	56,52	+0,11
30	55,85	—0,57	56,72	+0,31
Feb. 5	56,88	+0,46	57,68	+1,57
7	57,33	+0,91	58,11	+1,70
11	57,12	+0,70	57,88	+1,77
14	56,14	—0,28	56,85	+0,44
15	55,94	—0,48	56,63	+0,22

Table VI. Dublin Circle.	Ann. Var. 1815.	No. of Observations.	Jan. 1.	Mean N. P. D.	Reduction to 1819.	Mean N. P. D. Jan. 1, 1819.	
	″			° ′ ″	′ ″	° ′ ″	
Polaris.	—19,474	62 above, 74 below	1811	*1 1 42 0,80	—2 35,79	1 39 25,01	from Ob. 1809-1813
				- - - -	- - -	*3 1 39 25,05	from Ob. 1818-1821
Arcturus	+18,990	77	1811	*1 69 49 40,95	+2 31,92	69 52 12,87	from Ob. 1809-1813
		40	1814	*2 69 50 38,57	+1 34,93	13,40	from Ob. 1814
		259	- -	- - - -	- - -	*3 13,66	from Ob. 1818-1821
α Lyræ	— 2,984	126	1811	*1 51 23 6,65	— 23,87	51 22 42,78	from Ob. 1809-1813
		40	1814	*2 51 22 57,93	— 14,92	43,01	from Ob. 1814
		227	- -	- - - -	- - -	*3 42 84	from Ob. 1818-1821
α Aquilæ	— 9,033	76	1811	*1 81 37 17,30	—1 12,26	81 36 5,04	from Ob. 1809-1813
		45	1814	*2 81 36 50,34	45,19	5,15	from Ob. 1814
		320	- -	- - - -	- - -	*3 5,11	from Ob. 1818-1821
α Cygni	—12,580	47	1812	*1 45 23 10,77	—1 28,06	45 21 42,71	from Ob. 1810-1813
		22	1814	*2 45 22 45,44	1 2,91	42,53	from Ob. 1814
		142	- -	- - - -	- - -	*3 42,30	from Ob. 1818-1821

*1 Trans. R. I. Acad. Vol XII. p. 48-64.

*2 ——————— p. 119-125.

*3 Phil. Trans. 1821.

Explanations of, and Remarks relative to, the preceding Tables.

Table I. contains the difference of polar distances of γ Draconis and α Lyræ, reduced to January 1, 1815, from observations with the Greenwich mural circle, of *both* stars on *each* of 337 days from 1812 to 1816 inclusive. In the years 1812 and 1813 six microscopes were used, afterwards only two.

The greatest arc is that of September 16, 1816, and the least, that of July 21, 1812.

The former - - -	= 12° 53′ 52″,67.
The latter - - -	= 12 53 45 ,95.
The mean of 337 Observations	= 12 53 49 ,30.

Table II. contains the differences from the Greenwich observations of the polar distances of α Lyræ and Polaris, reduced to January 1, 1815, for three winters, together with the difference by the standard catalogue.

These arcs are discordant among themselves, and the last of them singularly differs from the standard catalogue.

The latter part of this Table exhibits the arcs when both stars were observed on the same day, in the winter 1815-1816. It is conceived there is no reason to expect that the arc, exceeding 150°, between the direct and reflected images of α Lyræ, can be more exactly measured than the arc, about 50°, between Polaris and α Lyræ.

Table III. The great irregularities that take place in the readings of the microscopes of the Greenwich circle, when there appears to be no cause for such, are very remarkable.

Part (1) of Table III. exhibits the difference between the microscope A and each of the other microscopes, on every other day, when α Lyræ was observed from July, 1813, to February, 1814.

The numbers in the *same* vertical column ought to have been equal had no derangements taken place. The index equation = — 0″,45, is stated to have been constant between July and November 1. Between November 1, and February 25, it increased gradually to —3″,60. But the discordances in each of the vertical columns seem not to have relation to the changes of the index equation; on the contrary the alterations, that appear to have taken place, when the index equation is supposed to have remained the same, are as great as when it was changed.

It may be said, if the relative positions of the microscopes remained the same for the day, no inconvenience could arise from their changing from one day to another. But what are the causes of these changes? How can the accuracy of an instrument be relied on, or be estimated, that admits of such changes? Besides, if we examine, the relative positions do not appear to remain the same for even a day, (2) (3) (4) exhibit the state of the differences for three several days: one in summer, one in autumn, and one in winter. Such discordances, it is true, are not found here as in (1) but they are much greater than could have been expected or ought to be. It may perhaps be supposed that *these* arise from errors of division, but it is not likely that errors of division have any great influence. Indeed it is probable that this instrument is more accurately divided than any one that has ever been constructed.

Table IV. This Table is constructed from observations of α Lyræ made with the Dublin circle. It exhibits the state of the side microscopes compared with the bottom microscope for about eight months (one season of the observations of α Lyræ.)

The readings from which these are deduced are given in my paper on solar nutation, printed for the XIVth Volume of the Transactions of the Royal Irish Academy, and of which paper copies have been for some time in the hands of several persons.

In part (1), the second column marked WL (meaning face of the circle west and left hand microscope) contains the differences between the bottom microscope and the left hand microscope for each day. The column marked WR contains the difference between the bottom and right hand microscope. The fourth column, WM, contains the mean of these differences for each day. These second and third columns show a great steadiness during such a length of time. There are no sudden changes, such as we meet with in the Greenwich circle. Nothing, I conceive, can be more remarkable than the contrast in this respect between the two instruments. In the Dublin circle, when a change appears to take place, it comes on gradually.

From the construction and manner in which the Dublin circle is supported, it is to be expected that changes may take place of the relative positions of the two microscopes to the bottom microscope, while the *mean* of the two microscopes will still preserve the same relation to the bottom microscopes. This is shown most satisfactorily in column 4, marked WM.

Part (2) contains the comparison of the microscopes when the face of the circle was east. The same consistency appear here as when the face was west. No stronger proof of the excellence of the Dublin instrument can be required than is exhibited by the columns WM and EM.

Considering the manner in which the microscopes are placed on the two instruments, theory must be in favour of the superior steadiness of those of the Greenwich instrument, but experience teaches us quite otherwise.

The last column of part (2) of this Table will, I conceive, appear highly worthy of attention. It is half the sum of the columns WM and EM, and therefore is the difference for *each* day between the zenith distances determined by the bottom microscope, and by the mean of the side microscopes.

It shows that the different temperatures of winter and summer do not operate a change in the figure of the circle, at least in an arc of 14°, and so in this respect can have no reference to the parallax of α Lyræ.

The same column also shows the great exactness of the readings, the very few instances of discordances in the column may be either attributed to slight temporary derangements in the microscopes, as is evidently shown, September 12, in column WR, or to the small errors of reading lying so as to appear with an accumulated effect.

If B, L, R represent the readings of the bottom, left, and right hand microscopes, when the face of the circle is west, and B′ L′ R′ the same when the face is east; the quantity in the last column of this Table, part (2) =

$$\frac{1}{2}\left(B - \frac{L+R}{2} + B' - \frac{L'+R'}{2}\right) = \frac{B+B'}{2} - \frac{1}{2}\left(\frac{L+L'}{2} + \frac{R+R'}{2}\right) =$$

the mean of the differences between the zenith distance by the bottom and each of the side microscopes respectively.

It will seldom happen for other stars that the number in the last column will be so very small. But the equality of the numbers is the circumstance to be reckoned on here, not their magnitude.

Table V, column 1, contains the mean Z. distance of α Lyræ, reduced to January 1, 1819, from each days observation with the Dublin circle, between July, 1821, and February, 1822, in which, besides the usual equations, the equation for parallax (const. 1″,1) is used, and also the constant of aberration is taken = 20″,35, conformably to the results in my paper on solar nutation, above referred to.

Column 2 contains the difference between this and the mean zenith distance 14° 45′ 56″,42.

Column 3 contains the mean zenith distance uncorrected for parallax, and taking the aberration = 20″.25.

Column 4 contains the difference between the 3rd column and the mean 14° 45′ 56″,41.

Table VI. contains the mean places of several stars obtained at different periods. These stars having been very frequently observed in consequence of the investigations about parallax, show the consistency of the instrument at these different periods.

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ACADEMIÆ et SOCIETATES.	
Magnæ Britanniæ.	
ROYAL INSTITUTION.—A Journal of Science, Literature and the Arts, edited at the Royal Institution of Great Britain, No. 30—32. 8° *London*, 1823	The Managers of the Royal Institution.
SOCIETY FOR THE ENCOURAGEMENT OF ARTS, MANUFACTURES AND COMMERCE.—Transactions of the Society instituted at London for the Encouragement of Arts, Manufactures, and Commerce, with the Premiums offered in the year 1822. Vol. XLI. 8° *London*, 1823	The Society for Encouragement of Arts, &c. Jan. 29, 1824.
MEDICAL AND CHIRURGICAL SOCIETY OF LONDON.—Medico-Chirurgical Transactions, published by the Medical and Chirurgical Society of London. Vol. XII. Part II. 8°. *London*, 1823	The Medical and Chirurgical Society, Nov. 20, 1823.
HORTICULTURAL SOCIETY. Transactions of the Horticultural Society of London, Vol. V. Part II. and III. 4°. *London*, 1823	The Horticultural Society, Nov. 20, 1823.
——— Report of the Garden Committee on the formation and progress of the Garden, drawn up for the information of the Fellows of the Society, as directed by the Bye-Laws. 4° *London*, 31st March, 1823.	———
CAMBRIDGE PHILOSOPHICAL SOCIETY. Transactions of the Cambridge Philosophical Society, Vol. II. Part I 4°.	The Cambridge Philosophical Society, June 3, 1824.
ROYAL SOCIETY OF EDINBURGH. Transactions of the Royal Society of Edinburgh, Vol. X. Part I. 4°. *Edinburgh*, 1824.	The Royal Society of Edinburgh, May 6, 1824.
Galliæ.	
ACADÉMIE DES SCIENCES ARTS ET BELLES-LETTRES DE DIJON. Académie des Sciences Arts et Belles-Lettres de Dijon, Séance publique du 24 Août, 1821. 8°. *Dijon*, 1822.	The Academy of Sciences and Literature of Dijon, Feb. 5, 1824.

PRESENTS.	DONORS.
ACADEMIÆ et SOCIETATES.	
ÉCOLE POLYTECHNIQUE. Journal de l'École Polytechnique, publié par le Conseil d'Instruction de cet Établissement, Tome XI. et XII. 4°. *à Paris*, 1820—23.	Le Conseil de l'École Polytechnique, Nov. 20, 1823.
Germaniæ.	
SOCIETAS REGIA SCIENTIARUM GOTTINGENSIS. Commentationes Societatis Regiæ Scientiarum Gottingensis recentiores Vol. V. An. D. 1819—22. 4° *Gottingæ*, 1823.	The Royal Society of Sciences at Gottingen, Jan. 22, 1824.
ACADEMIA LEOPOLDINO-CÆSAREA. Nova Acta Physico-Medica Academiæ Cæsareæ Leopoldino-Carolinæ Naturæ Curiosorum. Tom. I. Parts I. et II.	The Cæsarean Academy of Natural Historians at Bonn, May 27, 1824.
——— Verhandlungen der Kaiserlichen Leopoldinisch-Carolinischen Akademie der Naturforscher. 4° Bonn, 1823.	———
Russiæ.	
ACADEMIA SCIENTIARUM IMPERIALIS PETROPOLITANA. Mémoires de l'Académie Impériale des Sciences de St. Petersbourg Tom VIII. avec l'Histoire de l'Académie pour les Années 1817 et 18. 4° *St. Petersbourg*, 1822.	The Imperial Academy of Sciences of St. Petersbourgh, Nov. 20, 1823.
Italiæ.	
ACADÉMIE DES SCIENCES, &c. DE TURIN. Memorie della Reale Academia delle Scienze di Torino. Tom. XXV. et XXVI. 4° *Torino*, 1820—21.	The Royal Academy of Sciences of Turin, Nov. 20, 1823.
SOCIETA ITALIANA. Memorie di Matematica e di Fisica della Società Italiana delle Scienze residente in Modena. Tomo XIX. Parte contenente le Memorie di Fisica. 4° *Modena*, 1823.	La Società Italiana, March 25, 1824.
ALDERSON (JOHN) M. D. An Essay on Apparitions, in which their appearance is accounted for by causes wholly independent of preternatural agency. New edition, revised and corrected. 8° *London*, 1823.	Dr. John Alderson, Nov. 27, 1823.
AMPÈRE, (M.) Receuil d'Observations Electro-Dynamiques, contenant diverses Mémoires, Notices, Extraits de Lettres, ou d'ouvrages périodiques sur les Sciences, rélatifs à l'action mutuelle de deux courans électriques, à celle qui existe entre un courant électrique et un Aimant, ou le globe terrestre, et à celle de deux Aimans l'un sur l'autre. 8° *Paris*, 1822.	M. Ampère, May 20, 1824.
ANNALS OF PHILOSOPHY, New Series, No. 31–42 8° *London*, 1823.	Mr. R. Phillips.
ASTRONOMIA, *v.* BESSEL. HARDING. LITTROW. ROBINSON. STRUVE.	
——— Connaissance des Tems ou des Mouvemens Célestes à l'Usage des Astronomes et des Navigateurs pour l'An 1826, publiée par le Bureau des Longitudes. 8° *Paris*, 1823.	Le Bureau des Longitudes de France, April 1, 1824.

PRESENTS.	DONORS.
ASTRONOMIA. Astronomisches Jahrbuch für das Jahr 1825, nebst einer Sammlung der neusten in die astronomisches Wissenschaften einschlagenden Abhandlungen, Beobachtungen, und Nachrichten. Von. J. E. Bode. 50 Band. 8° *Berlin*, 1822.	Professor Bode, Nov. 20, 1823.
——————Astronomische Nachrichten, No. 33-57. 4°.	Professor H.C. Schumacher, Nov. 20, 1823.
——————Astronomische Hülfstafeln für 1823-1824. 8° *Copenhagen*.	——————
BARLOW (PETER) An Elementary Investigation of the Theory of Numbers, with its application to the indeterminate and Diophantine Analysis, the analytical and geometrical division of the Circle, and several other curious Algebraical and Arithmetical Problems. 8° *London*, 1811.	Professor Barlow, May 13, 1824.
——— New Mathematical Tables, containing the factors, squares, cubes, square roots, cube roots, reciprocals, and hyperbolic Logarithms of all Numbers from 1 to 10,000; Tables of powers and prime numbers, an extensive Table of Formulæ, or general Synopsis of the most important particulars relating to the doctrines of Equations, Series, Fluxions, Fluents, &c. &c. &c. 8° *London*, 1814.	——— ———
——— An Essay on Magnetic Attractions and on the Laws of Terrestrial and Electro-Magnetism; comprising a popular course of curious and interesting Experiments on the latter subject, and an easy Experimental Method of correcting the Local Attraction of Vessels on the Compass in all parts of the World. 8° *London*, 1823.	——————
——— An Essay on the Strength and Stress of Timber, founded upon Experiments performed at the Royal Military Academy, on Specimens selected from the Royal Arsenal and His Majesty's Dock Yard, Woolwich, preceded by an Historical Review of former Theories and Experiments, with numerous Tables and Plates; also, an Appendix on the Strength of Iron and other materials. 8° *London*, 1824.	——————
BELL, (THOMAS) *v.* ZOOLOGICAL JOURNAL.	
BENOIT, (P. M. N.) Théorie génerale des Pèse-Liqueurs appliquée à la construction et à l'emploi de toutes sortes d'aréomètres entièrement comparables; avec des Tables aréométriques très étendues, donnant les pesanteurs spécifiques correspondant aux divers degrés des Pèse-Liqueurs en usage; le titre des Eaux-de-vie, des Acides sulfuriques, &c. 8° *à Paris*, 1821.	M. Benoit, Nov. 20, 1823.
BERTHEVIN et TREUIL, (*MM.*) Élémens d'Arithmétique Complémentaire, ou Méthode Nouvelle par laquelle à l'aide des Complémens Arithmétiques on exécute toutes les opérations de Calcul. 8vo. *à Paris*, 1823.	MM. Berthevin et Treuil, Nov. 20, 1823.
BESSEL (F. W. VON) Astronomische Beobachtungen auf der Königlichen Universitäts Sternwarte in Königsberg. Siebente Abtheilung — vom 1 Januar bis 31 December, 1821. fol. *Königsberg*, 1822.	F. W. von Bessel, Nov. 20, 1823.

PRESENTS.	DONORS.
BESSEL (F. W. VON) Achte Abtheilung, vom 1 Januar bis 31 December. fol. *Königsberg*, 1823.	F. W. von Bessel, Nov. 20, 1823.
BODE, *v.* ASTRONOMIA.	
BOSTOCK (JOHN) M. D. An Elementary System of Physiology, Vol. I. 8° *London*, 1824.	Dr. John Bostock, April 29, 1824.
BOURNON (*M. le Comte* DE) Observations sur quelques-uns des Minéraux soit de l'Ile de Ceylan, soit de la Côte de Coromandel rapportés par M. Leschenault de Latour. 4° *Paris*, 1823.	M. le Comte de Bournon, Nov. 20, 1823.
BRANDES (*Prof.* ENRICO GUGLIELMO) Considerazioni sull' Abassamento straordinario del Barometro nel dì 25 Decembre, 1821, del Sig. Prof. Enr. Gug. Brandes Breslavia spedite alla Società Italiana residente in Modena, tradotte dal Tedesco. 4°	Professor E. G. Brandes, March 25, 1824.
BRESCHET, EDWARDS et VAVASSEUR *(MM.)* Recherches Expérimentales sur les Fonctions du Système Nerveux, Premier Mémoire, de l'Influence du Système Nerveux sur la Digestion stomachale. 8°.	MM. Breschet, Edwards, et Vavasseur, June 5, 1823.
BRIDGEWATER (*Rev. Earl of*) Note (C) indicated at page 113 in the third Part of the Letter on Inland Navigation to the Parisians and French Nation. 8° *No date*.	The Rev. the Earl of Bridgewater, March 18, 1824.
BUCK, (JOHN) A new General and Algebraical Solution of the higher orders of Equations, with Solutions of Equations, to the tenth degree inclusivè. 8° *London*, 1823.	Mr. John Buck, Nov. 20, 1823.
CATALOGUS. An Account of Greek Manuscripts, chiefly Biblical, which had been in the possession of the late Professor Carlyle, the greater part of which are now deposited in the Archiepiscopal Library at Lambeth Palace. 8° *London, no date*.	Archbishop of Canterbury, Nov. 23, 1823.
CHARTS, *v.* MAPS AND CHARTS.	
CHEVALIER (THOMAS) F. R. S. Lectures on the General Structure of the Human Body, and on the Anatomy and Functions of the Skin, delivered before the Royal College of Surgeons in London, in the Courses for 1823. 8° *London*, 1823.	Thomas Chevalier, Esq. Nov. 27, 1823.
CHEVREUL (M. E.) Considérations Générales sur l'Analyse Organique et sur ses applications. 8° *à Paris*, 1824.	M. Chevreul, May 20, 1824.
———— Recherches Chimiques sur les Corps Gras d'Origine Animale, 8° *Paris*, 1823.	————
CHILDREN (JOHN GEORGE) *v.* ZOOLOGICAL JOURNAL.	
CIVIALE (LE *Docteur*) Rapport fait à l'Académie Royale des Sciences, par MM. le Chevalier Chaussier et le Baron Percy sur le Nouveau Moyen du Docteur Civiale pour détruire la pierre dans la Vessie sans l'opération de la taille. 8° *Paris*, 1824.	D. Civiale, May 27, 1824.
CONNAISSANCE DES TEMS, *v.* ASTRONOMIA.	
CONWELL, (W. E. E.) M. D. *de Londonderry, (Irlande)*. Recherches sur les Propriétés médicinales et l'emploi en Médecine de l'Huile de Croton Tiglium, et quelques Propositions sur les Maladies de l'Inde. 4° *Paris*, 1824.	Dr. W. E. E. Conwell, Apr. 29, 1824.
DANIELL, (JOHN FRED.) F. R. S. Meteorological Essays and Observations. 8° *London*, 1823.	J. F. Daniell, Esq. Nov. 20, 1823.

PRESENTS.	DONORS.
DELEUZE, (M.) History and Description of the Royal Museum of Natural History, published by Order of the Administration of that Establishment, translated from the French of M. Deleuze, Part II. 8° *Paris*, 1823.	Mr. Royer, Nov. 20, 1823.
DESMOULINS, (M. A.) M. D. Sur l'État Anatomique de la Peau et du Tissu cellulaire sous-cutané dans la Fièvre Jaune. 8°	M. Desmoulins, June 17, 1824.
——————— Mémoire sur le défaut d'unité de composition du Système nerveux, et sur la concordance de ce défaut d' unité avec l'inégalité des Facultés des Animaux. 8°.	
——————— Mémoire sur la distribution géographique des Animaux vertèbres, moins les oiseaux, lu à la première Classe de l'Institut, le 25 Fevrier, 1822. 4°.	———————
——————— Exposition des motifs d'un nouveau système d'Hygiène deduit des lois de la physiologie, et appliqué au perfectionnement physique et moral de l'homme. 4° *Paris*, 1818.	———————
——————— Mémoire sur le Rapport qu'a l'étendue des Surfaces de la Rétine et du Nerf optique des oiseaux avec l'énergie et la portée de leur vue. Présentée à l'Académie des Sciences de l'Institut le 23 Dec. 1822. 8°.	———————
——————— Mémoire sur le Rapport qui unit le développement du Nerf Pneumo-Gastrique à celui des Parois du quatrième ventricule, et sur la composition de la Moelle Épinière. Lu à l'Institut le 4 Août, 1823. 8°.	———————
——————— Sur la Patrie du Chameau à une bosse, et sur l'époque de son Introduction en Afrique. Mémoire lu à l'Académie des Inscriptions et Belles-Lettres de l'Institut le 28 Juin, 1823. 4°.	———————
——————— De l'État du Système Nerveux sous ses rapports de volume et de masse dans le marasme non sénile, et de l'influence de cet état sur les fonctions nerveuses. Lu à l'Académ. Royale des Sciences, 29 Mai, 1820. 4°.	———————
DOUGLAS, (*Col. Sir* HOWARD *Bart.*) Observations on the Motives, Errors and Tendency of M. Carnot's Principles of Defence, showing the defects of his new System of Fortification, and of the alterations he has proposed, with a view to improve the defences of existing Places. 8 *London*, 1819.	Col. Sir Howard Douglas, Bart. Nov. 20, 1823.
DUPIN (CHARLES) Applications de Géométrie et de Mécanique à la Marine, aux Ponts et Chaussées, &c. et pour faire suite aux développements de Géometrie. 4° *Paris*, 1824.	M. Dupin, May 6, 1824.
EMILIANI (LUIGI), M. D. Risposta al Tema proposto con Programma 22 Luglio, 1821, dalla Società Italiana delle Scienze residente in Modena. Memoria coronata dalla Società medesima. 4° *Modena*, 1823.	Dr. L. Emiliani, March 25, 1824.

PRESENTS.	DONORS.
EDWARDS (H. MILNE) M. D. Mémoire sur la Structure Élémentaire des principaux Tissus organiques des Animaux. 4° *à Paris*, 1823.	Dr. H. Milne Edwards, Jan. 15, 1824.
EDWARDS (W. F.) M. D. De l'Influence des Agens Physiques sur la vie. 8° *Paris*, 1824. *v.* BRECHET.	Dr. W. F. Edwards, April 8, 1824.
THE EUROPEAN MAGAZINE AND LONDON REVIEW. June–October, 1823. 8° *London*, 1823.	The Proprietors.
FLORA BATAVA. No. 63–65. 4°	H. M. The King of the Netherlands.
FORSTER (THOMAS) M. D. Somatopsychonoologia, showing that the proofs of Body Life and Mind considered as distinct essences cannot be deduced from Physiology, but depend on a distinct sort of Evidence, being an Examination of the Controversy concerning Life, carried on by MM. Laurence, Abernethy, Rennell, and others, by Philostratus. 8° *London*, 1823.	Dr. Thomas Forster, March 11, 1824.
HALL (*Capt.* BASIL, *R. N.*) Extracts from a Journal written on the Coasts of Chili, Peru, and Mexico, in the years 1820, 1821, and 1822. 2nd Edition, 2 Vols. 12° *Edinburgh*, 1824.	Captain B. Hall, June 17, 1824.
HARDING (CAROLUS LUDOVICUS) Atlas Novus Cœlestis XXVII. Tabulis, continens Stellas inter Polum Borealem et trigesimum gradum Declinationis Australis adhuc observatas. 27 sheets. *Gottingæ*, 1822.	M. C. L. Harding, May 6, 1824.
HOME (*Sir* EVERARD, *Bart.*) Lectures on Comparative Anatomy, in which are explained the preparations in the Hunterian Collection, illustrated by Engravings, to which is subjoined Synopsis Systematis Regni Animalis nunc primum ex ovi modificationibus præpositi. Vol. III. and IV. 4° *London*, 1823.	Sir E. Home, Bart. Nov. 20, 1823.
HORSBURGH, JAMES, *v.* MAPS AND CHARTS.	
HUBERUS (DANIEL) Nova Theoria de Parallelarum Rectarum proprietatibus. 12° *Basiliæ*, 1823.	Mr. Huber, Jan. 29, 1824.
——————— Eröffnungs-Rede de siebenten Jahres-Versammlung der Allgem. Schweizerischen Gesellschaft für gesammte Naturwissenschaften gehalten in Basel den 23 heumonats 1821. 12° *Basel*, 1821.	———————
HUMBOLDT (*Baron* DE) Selections from the Works of the Baron de Humboldt relating to the Climate, Inhabitants, Productions, and Mines of Mexico, with Notes by John Taylor, Esq *Tr. Geol. Soc. London*, 1824.	John Taylor, Esq. April 29, 1824.
JOMARD (*M.*) Voyage à l'Oasis de Syouah, redigé et publié d'après les matériaux recueillis par M. le Chev. Drovetti, Consul Général de France en Égypte, et par M. Frédéric Caillaud de Nantes pendant leurs voyages dans cette Oasis en 1819 et en 1820. 2 et 3 Livraisons. fol. *Paris*, 1823.	M. Jomard, Nov. 20, 1823.
——————— Recueil d'Observations et de Mémoires sur l'Égypte Ancienne et Moderne, ou Description historique et pittoresque de plusieurs des principaux monumens de cette contrée, accompagnée de Recherches sur les Connaissances des anciens Égyptiens et de Remarques sur la Géographie, l'Archæologie, et les Beaux-Arts. Tome 3e. 8°. *Paris, no date.*	———————

PRESENTS.	DONORS.
IRBY (*the Hon.* CHARLES LEONARD) and JAMES MANGLES (*Commanders in the Royal Navy.*) Travels in Egypt and Nubia, Syria and Asia Minor during the years 1817 and 1818. 8° *London*, 1823.	Captains Irby and Mangles, Dec. 18, 1823.
KIRCKHOFF (LE *Chev.* J. ROM. LOUIS DE) Hygiène Militaire à l'Usage des Armées de Terre. 8° *à Anvers*, 1823.	Le Chev. Kirckhoff, Nov. 20, 1823.
LANCASTRIÆ DUCATUS. Pars prima. Calendarium Inquisitionum post mortem, &c. temporibus Regum Edw. I. Edw. III. Rich. II. Hen. V. Hen. VI. Edw. IV. Hen. VII. Hen. VIII. Edw. VI. Regin Mar. Phil. et Mar. Eliz. Jac. I. Car. I. Pars Secunda. A Calendar to the pleadings, &c. in the Reigns of Hen. VII. Hen. VIII. Edw. VI. Queen Mary and Phil. and Mary, printed by Command of H. M. King George IV. in pursuance of an Address to the House of Commons of Great Britain. fol. 1823.	The Commissioners of Public Records, Nov. 20, 1823.
LAPLACE (M LE *Marquis* DE LA.) Traité de Mécanique Céleste. Livre 13e 4° *Paris*.	The Marquis de Laplace, Feb. 19, 1824.
———— Exposition du Système du Monde. 5e Édition revue et augmentée par l'auteur. 4° *Paris*, 1824.	———— Feb. 26, 1824.
LATREILLE (*M.*) Recherches Géographiques sur l'Afrique Centrale, d'après les Écrits d'Édrisi et de Léon l'Africain, comparés avec les Relations Modernes. 8° *Paris*, 1824.	M. Latreille, May 20, 1824.
LITTROW (J. J.) Annalen der K. K. Sternwarte in Wien nach dem Befehle Seiner Majestat auf offentliche kosten herausgegeben. Dritter Theil. fol. *Wien*, 1823.	Professor J. J. Littrow, Nov. 20, 1823.
LOCKER (EDWARD HAWKE) Views in Spain from Sketches made in a Tour through that Kingdom in the Autumn of 1813. No. 2–10. 4° *London*, 1823.	Edward Hawke Locker Nov. 20, 1823.
LYALL (ROBERT) M. D. An Essay on the Chemical and Medicinal Qualities of the Water of Candren Well in Renfrewshire, with introductory Observations on Waters in general. 8° *Paisley*, 1814.	Dr. Robert Lyall, March 11, 1824.
———— Dissertatio Inauguralis Medico-Chirurgica de Staphylomate pellucido-Conico. 8° *Petropoli*, 1816.	————
———— An Account of the Organization, Administration, and present State of the Military Colonies in Russia, with an Appendix containing Statistical Tables, &c. &c. 8° *London*, 1824.	———— Feb. 12, 1824.
MACKENZIE (GEORGE) A Brief Account of the Cycle of the Lunes, by the Author of the Cycle of the Weather. 8° *Perth*, 1824.	Mr. George Mackenzie, April 29, 1824.
MAIDEN (WILLIAM) An Account of a Case of Recovery after the Shaft of a Chaise had been forced through the Thorax, to which is now added, a Statement of the health of the sufferer from the period of his recovery until his decease, with the appearances of the injured parts after death. 4° *London*, 1824.	William Maiden, Esq. April 8, 1824.

PRESENTS.	DONORS.
MAJENDIE (F) Formulaire pour la préparation et l'emploi de plusieurs nouveaux Médicamens, tels que la Noix vomique, les Sels de Morphine, l'Acide prussique, la Strychnine, la Vératine, les Alcalis des Quinquinas, l'Émétine, l'Iode, l'Iodure de Mercure, le Cyanure de Potassium, l'Huile de Croton Tiglium, &c. &c. &c. 12° *à Paris*, Juin, 1824.	M. Majendie, June 17, 1824.
——— Journal de Physiologie Expérimentale et Pathologique. 2e Numero Mai, 1824. Tome 4. 8° *Paris*, 1824.	——— ———
MANGLES (JAMES) *v.* IRBY.	
MAPS AND CHARTS. Eastern Passages to China, Sheet 1, by James Horsburgh, Esq. *London*, 1 May, 1824.	James Horsburgh, Esq. May 13, 1824.
——— Survey of the Straits of Durian by Lieutenants W. S. Collinson, Hawkins and Moresby. 1 Sheet, *London*, 1 Nov. 1823.	———
——— The East Coast of Sumatra from Diamond Point to the Southern Entrance of Brewer's Straits, by Lieut. William Rose and Robert Moresby, H. E. I. C. Marine. 1 Sheet. *London*, 1 Jan. 1824.	——— ———
THE MONTHLY REVIEW ENLARGED, from June, 1823, to May, 1824, and Appendix to Vol. CI. CII. CIII. 8° *London*, 1823.	George Edward Griffiths, Esq.
MORRISON (R.) D. D. A Dictionary of the Chinese Language, in three Parts, Part the 1st containing Chinese and English, arranged according to the Radicals, Part the 2nd, Chinese and English arranged Alphabetically, and Part the 3rd, English and Chinese. Vol. II. Part. 1. 4° *London*, 1822.	The Court of Directors of the Hon. East India Company, Nov. 20, 1823.
PARRY (*Captain* W. E.) A Supplement to the Appendix of Captain Parry's Voyage for the Discovery of a North-west Passage, in the Years 1819-20, containing an Account of the Subjects of Natural History. 4° *London*, 1824.	Captain W. E. Parry, Feb. 5, 1824.
——— Journal of a Second Voyage for the Discovery of a North-west Passage from the Atlantic to the Pacific, performed in the Years 1821, 22, 23, in H. M. SS. Fury and Hecla, under the Orders of Captain W. E. Parry, R. N. F. R. S. illustrated by numerous Plates. 4° *London*, 1824.	——— ——— March 25, 1824.
PEARSON (*Rev.* WILLIAM,) LL. D. F. R. S. An Introduction to Practical Astronomy, containing Tables recently computed for facilitating the Reduction of Celestial Observations, and a popular explanation of their Construction and Use. Vol. I. 4° *London*, 1824.	Rev. Dr. William Pearson, April 29, 1824.
THE PHILOSOPHICAL MAGAZINE AND JOURNAL. No. 302-313. 8° *London*, 1823.	Dr. Alexander Tilloch.
PREVOST (*M.*) et M. DUMAS, Mémoire sur les Phénomènes qui accompagnent la contraction de la fibre Musculaire. Lu à l'Académie des Sciences 18 Aout, 1823. 8° *Paris*, 1823.	Dr. Prevost, May 20, 1824.

PRESENTS.	DONORS.
PREVOST *(M.)* et *M.* DUMAS. Nouvelle Théorie de la Géneration. 8° *no date or place.*	Dr. Prevost, May 26, 1824.
J. L. PREVOST, *M. D. et* J. A. DUMAS. Examen du Sang et de son Action dans les divers Phénomènes de la vie. 8°	M. J. L. Prevost, June 3, 1824.
ROBERTSON, (*Rev.* ABRAM) D.D. Astronomical Observations made at the Radcliffe Observatory at Oxford, from May 1, 1823, to May 1, 1824, by and under the direction of the Rev. Abram Robertson, D. D. F. R. S. Sav. Prof. of Astron. and Radcliffe Observer. MS. fol.	The Trustees under the Will of the late Dr. Radcliffe, June 3, 1824.
RONALDS (FRANCIS) Descriptions of an Electrical Telegraph, and some other Electrical Apparatus. 8° *London,* 1823.	Mr. F. Ronalds, Nov. 20, 1823.
SAVARY *(M. F.)* Mémoire sur l'Application du Calcul aux Phénomènes Électro-Dynamiques. 4° *Paris,* 1823.	M. Savary, Nov. 20, 1823.
SCHUMACHER *(Prof.* H. C.*) vide* ASTRONOMIA. *(Nachrichten.)*	
SCUDAMORE (CHARLES) M. D. F R. S. An Essay on the Blood, comprehending the chief circumstances which influence its Coagulation, the nature of the Buffy Coat, with a concise medical view of the State of the Blood in Disease, and an Account of the powers of a saturated Solution of Alum as a Styptic Remedy in Hemorrhage. 8° *London,* 1824.	Dr. Charles Scudamore, March 11, 1824.
SMEATON (JOHN) A Portrait, in Oil, of the late John Smeaton, Esq. F. R. S.	Bequeathed by Mrs. Dixon, his Daughter, April 29, 1824.
SMITH *(Sir* JAMES EDWARD*)* M. D. F. R. S. The English Flora, in 2 Vols. 8° *London,* 1824.	Sir James Edward Smith, March 18, 1824.
SODA (DIONIGIO) La Verità Rettificatrice ritrovata. 8° *Napoli,* 1816.	Dionigio Soda, Jan. 15, 1824.
——— ——— La Geometria Piana rivendicatrice. 8° *Napoli,* 1822	———
SOWERBY (JAMES DE CARL) and G. B. *vide* ZOOLOGICAL MAGAZINE.	
STATUTES. The Alphabetical Index to the Statutes of the Realm, from Magna Charta to the end of the Reign of Queen Anne. fol. 1824.	The Commissioners of Public Records, May 27, 1824.
STRUVE (F. G. W.) Observationes Astronomicæ institutæ in Specula Universitatis Cæsareæ Dorpatensis. Vol. II. and III. 4° *Dorpat,* 1820	F. G. W. Struve, Nov. 20, 1823.
TANTINI (FRANCESCO) M. D. Principj d'Instituzioni Cliniche, ossia Introduzione alla pratica clinica per uso dei principiânti di Gio Valentino Nob. de Hildenbrand, versione dal Latino del Dot. Fr. Tantini. 8° *Pisa,* 1818.	Professor Tantini, Nov. 20, 1823.
TAYLOR (JOHN) *vide* HUMBOLDT.	
TIEDEMANN (FREDERIC) Anatomie du Cerveau, contenant l'histoire de son Développement dans le Fœtus avec une exposition comparative de sa structure dans les Animaux, par Fr. Tiedemann, traduite de l'Allemand avec un discours préliminaire sur l'Etude de Physiologie en général et sur celle de l'Action du Cerveau en particulier, par J. L. Jourdan, M. D. avec 14 planches. 8° *Paris,* 1823.	Dr. J. L. Jourdan, Nov. 20, 1823.

PRESENTS.	DONORS.
TINCHANT (M.) Doctrine Nouvelle sur la Réproduction de l'Homme, suivie du Tableau des variétés de l'Espèce humaine. 8° *Paris*, 1822.	M. Tinchant, May 27, 1824.
——— De la Digestion et des Phénomènes qui se succèdent dans les organes digestifs pendant l'acte de l'assimilation ou de la nutrition, Réponse à la Question proposée par l'Institut Royal de France pour Prix de l'Année 1825. 8° *Paris, Avril* 1824.	———
TOOKE, (THOMAS, *Esq.*) F. R. S. Thoughts and Details of the high and low prices of the thirty years, from 1793 to 1822, in four parts, Second Edition. 8° *London*, 1824.	Thomas Tooke, Esq. June 17, 1824.
TURNOR (EDMUND, *Esq.*) F. R. S. F. S. A. A short View of the Proceedings of the several Committees and Meetings held in consequence of the intended Petition to Parliament from the County of Lincoln, for a limited Exportation of Wool in the year 1781 and 1782, together with Mr. R. Glover's Letter on that subject; to which is added, a List of the Pamphlets on Wool lately published, with some Extracts. Extracted from the Pamphleteer, No. XLVI. 4° London, printed 1782, reprinted 1824.	Edmund Turnor, Esq. May 20, 1824.
VALENTIN (*Dr.* LOUIS) Notice Historique sur le Docteur Jenner, suivie de Notes relatives à sa découverte de la Vaccine. 8° *à Nancy*, 1823.	Dr. L. Valentin, Nov. 20, 1823.
VAVASSEUR, *v.* BRECHET.	
YEATS (G. D.) M. D. F. R. S. A Statement of the early Symptoms which lead to the Disease termed Water in the Brain, with Observations on the necessity of a watchful attention to them, and on the fatal consequences of their neglect. Second Edition, considerably enlarged. 8° *London*, 1823.	Dr. G. D. Yeats, Nov. 20, 1823.
YOUNG (*Dr.* THOMAS, F. R. S.) Tides, from the Supplement to the Encyclopædia Britannica, Vol. VI. Part II. 4°.	Dr. Thomas Young, March 11, 1824.
THE ZOOLOGICAL JOURNAL, No. I. conducted by Thomas Bell, John George Children, James de Carle Sowerby, and G. B. Sowerby, Esq. 8° *London*, 1824.	Thomas Bell, Esq. John George Children, James de Carle Sowerby, and G. B. Sowerby, Esq. Feb. 26, 1824.

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The references to the Figures in Mr. IVORY's Paper having been accidentally omitted, are to be thus supplied;

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METEOROLOGICAL JOURNAL

for January, 1823.

1823 January.	Time. H.	Time. M.	Barometer corrected. Inches.	Six's Therm. °	Therm. without. °	Daniell's Hygrom. °	Degree of Moisture.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
☿ 1	9	0	29,768	28	32	19	645		W	1	Thick Fog.
	3	0	29,763	33	33	21	652		SE	1	Cloudy and hazy.
♃ 2	9	0	29,721	32	39	33	823		SSE	1	Cloudy.
	3	0	29,771	43	43	38	835		S	1,2	Rain.
♀ 3	9	0	29,819	40	41	36	849	0.220	E	1	Fine.
	3	0	29,806	45	44	36	756		E	1	Cloudy.
♄ 4	9	0	29,754	41	41	33	767		E	1	Cloudy.
	3	0	29,721	45	41	35	822		E	1	Cloudy.
☉ 5	9	0	29,745	40	41	32	931		E	1	Rain.
	3	0	29,715	41	40	39	772		E	1	Cloudy.
☽ 6	9	0	29,904	41	43	38	868		S	1	Cloudy.
	3	0	29,985	44	44	41	890	0.135	SSE	1	Cloudy.
♂ 7	9	0	30,138	39	41	36	849		E	1	Cloudy.
	3	0	30,132	43	41	32	740		Var.	1	Cloudy.
☿ 8	9	0	30,181	36	37	33	875		NNE	1	Cloudy.
	3	0	30,157	40	38	33	848		N	1	Fine, though rather hazy.
♃ 9	9	0	30,021	31	32	27	843		E	1	Hazy.
	3	0	29,732	37	35	32	900		E by S	1	Fine.
♀ 10	9	0	29,881	32	34	28	810		E	1	Cloudy.
	3	0	29,866	35	35	29	808		N	1	Cloudy.
♄ 11	9	0	29,932	29	30	30	1000		N	1	Cloudy.
	3	0	29,931	30	35	23	658		NE	1	Fine.
☉ 12	9	0	29,889	26	27	16	676		N by E	1	Cloudy.
	3	0	29,851	31	30	14	575		NNE	1	Cloudy.
☽ 13	9	0	29,788	29	29	22	784		N	1	Snow.
	3	0	29,635	30	30	16	615		N	1	Cloudy.
♂ 14	9	0	29,590	26	27	13	610		E	1	Hazy.
	3	0	29,573	32	31	18	630		E	1	Cloudy.
☿ 15	9	0	29,260	24	25	20	824		E	1	Snow.
	3	0	29,278	31	31	18	630		N	1	Fine.
♃ 16	9	0	29,382	29	31	29	938		NW	1	Snow.
	3	0	29,369	32	31	23	760		NW	1	Cloudy.

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♀ 17	9	0	29,410	*	34	21	629			1	Cloudy.
	3	0	29,401		33	25	759		NNW	1	Cloudy.
♄ 18	9	0	29,379		32	25	787		NNW	1	Hazy.
	3	0	29,381		34	22	655		N	1	Cloudy.
☉ 19	9	0	29,508		22	11	678		N by W	1	Cloudy and hazy.
	3	0	29,524		27	14	632		NW	1	Thick Fog.
☽ 20	9	0	29,719		23	14	728		NNE	1	Hazy and cloudy.
	3	0	29,734		28	22	808		N	1	Hazy and cloudy.
♂ 21	9	0	29,887		31	25	817		NE	1	Cloudy.
	3	0	29,896		32	26	815		N by E	1	Cloudy.
☿ 22	9	0	29,981		28	22	808		N	1	Cloudy and hazy.
	3	0	29,975		28	18	697		N	1	Cloudy.
♃ 23	9	0	29,831		26	11	585		E	1	Cloudy.
	3	0	29,730		27	15	654		E	1	Fine, though rather hazy.
♀ 24	9	0	29,777		29	14	593		E by N	1	Cloudy.
	3	0	29,823		29	12	930		NE	1	Cloudy.
♄ 25	9	0	29,829		27	17	698		E	1,2	Cloudy.
	3	0	29,697		28	23	840		E	1	Sleet. [in the night.
☉ 26	9	0	29,795		28	21	777		N	1	Snow; a heavy fall of snow
	3	0	29,774		31	22	731		E	1	Cloudy.
☽ 27	9	0	29,695		31	25	817		E	1	Cloudy.
	3	0	29,486		34	28	810		E by S	1	Cloudy.
♀ 28	9	0	29,492		36	32	871		SW	1	Cloudy.
	3	0	29,639		40	38	971	0.250	SW	1	Cloudy.
☿ 29	9	0	29,124		43	40	886		S	1	Cloudy.
	3	0	29,066		45	39	800		S	1	Cloudy.
♃ 30	9	0	29,462		45	39	800		S	1	Cloudy.
	3	0	29,454		47	43	868		SW	1	Cloudy.
♀ 31	9	0	29,154		46	42	927		E	1	Cloudy.
	3	0	29,047		44	41	890		E	1	Cloudy.

* Six's Thermometer deranged, and a Horizontal Register Thermometer substituted for it.

METEOROLOGICAL JOURNAL

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1823 February.		Time.		Barometer corrected.	Register Therm.	Therm. without.	Daniell's Hygrom.	Degree of Moisture.	Rain.	Winds.		Weather.
		H.	M.	Inches.	°	°	°		Inches.	Points.	Str.	
♄	1	9	0	28,740	37	38	32	817		N	1	Rain.
		3	0	28,743		37	33	875		NE	1	Rain.
☉	2	9	0	28,586	36	39	36	912		E	1	Cloudy and hazy.
		3	0	28,610		43	41	924		E	1	Cloudy. [at ½ past 9 A.M.
☽	3	9	0	28,905	41	41	39	740		NE	1	Cloudy, a most intense fog
		3	0	29,167		42	39	895		NE	1	Cloudy.
♂	4	9	0	29,330	33	35	23	658		W	1	Fine.
		3	0	29,335		35	26	730		W	1	Fine.
☿	5	9	0	29,690	29	31	22	731		NW	1	Fine.
		3	0	29,759		35	27	758		N	1	Fine.
♃	6	9	0	29,590	31	31	20	673		E	1,2	Cloudy.
		3	0	29,461		31	25	817		E by S	1	Snow.
♀	7	9	0	29,186	32	34	31	896	0.785	E	1	Rain.
		3	0	29,126		43	41	924		S	1	Rain.
♄	8	9	0	29,488	35	35	32	900		W	1	Fine.
		3	0	29,520		41	32	740		W	1	Cloudy.
☉	9	9	0	29,668	34	38	32	818		W	1	Fine.
		3	0	29,687		43	35	760		W	1	Cloudy.
☽	10	9	0	29,420	39	44	39	829	0.131	W	1,2	Rain.
		3	0	29,108		48	45	904		SW	2	Rain.
♂	11	9	0	29,427	41	46	37	727		SW	1,2	Rain.
		3	0	29,361		50	46	880		W	2	Cloudy.
☿	12	9	0	29,318	46	46	40	795		W	1	Rain.
		3	0	29,306		48	41	777		W	1	Rain.
♃	13	9	0	29,619	35	38	31	788	0.201	W	1	Fine.
		3	0	29,639		46	36	705		SW	1	Fine.
♀	14	9	0	29,382	40	42	37	842		S	2	Fine.
		3	0	29,430		43	37	810		S	2	Fine.
♄	15	9	0	29,835	36	39	37	941		N	1,2	Rain.
		3	0	29,933		40	37	914		N	1	Rain.

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		H.	M.	Inches.	°	°	°		Inches.	Points.	Str.	
☉	16	9	0	30,158		44	33	863	0.270	N by E	1	Cloudy.
		3	0	30,204		40	35	857		N	1	Cloudy.
☾	17	9	0	30,163	35	35	35	1000		NE	1	Cloudy, dark weather.
		3	0	30,075		35	27	758		E	1	Cloudy.
♂	18	9	0	29,749	34	37	32	844		E	1	Cloudy.
		3	0	29,530		41	36	849		S	2	Cloudy.
☿	19	9	0	29,148	40	41	35	822		W	1	Fine.
		3	0	29,348		44	37	781		W	1	Fine.
♃	20	9	0	29,826	34	36	33	787		W	1	Fine.
		3	0	29,905		44	32	658		NW	1	Fine.
♀	21	9	0	29,545	38	44	42	927	0.102	SW	2	Rain.
		3	0	29,319		49	46	907		W	2	Rain.
♄	22	9	0	29,510	40	46	42	864		W by N	2	Rain.
		3	0	29,463		47	40	769	0.100	W by N	1	Cloudy.
☉	23	9	0	29,670		41	39	932		S	1	Rain.
		3	0	29,335		46	42	864		S	2	Rain.
☾	24	9	0	29,485	41	44	37	781	0 392	N	1	Fine.
		3	0	29,634		49	35	619		W	1	Fine.
♀	25	9	0	29,597	39	41	37	877		S	1	Rain.
		3	0	29,188		46	42	864		SSW	1	Cloudy.
☿	26	9	0	29,089	34	39	32	794		W	1	Fine.
		3	0	28,997		41	33	767		W by N	1	Fine.
♃	27	9	0	29,081	33	37	31	813		N	1	Fine.
		3	0	29,169		42	31	684		N	1	Fine.
♀	28	9	0	29,455	34	40	32	771		N	1	Cloudy.
		3	0	29,524		43	29	614		N	1	Cloudy.

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1823 March.		Time. H.	M.	Barom. corrected. Inches.	Register Therm. °	Therm. without. °	Daniell's Hygrom. °	Degree of Moisture. °	Rain. Inches.	Winds. Points.	Str.	Weather.
♄	1	9	0	29,870	33	37	33	875		NE	1	Cloudy and hazy.
		3	0	29,970						N	1	Cloudy.
☉	2	9	0	30,031	33	38	33	848	0.397	W	1	Cloudy.
		3	0	29,947		47	33	616		W	1	Cloudy.
☾	3	9	0	29,637	43	45	38	775		W	1	Cloudy.
		3	0	29,494		51	47	968		W	1	Cloudy.
♀	4	9	0	29,306	40	45	28	553		W	2	Fine, a violent gale at 4 A.M.
		3	0	29,299		46	27	517		N W	2	Fine.
☿	5	9	0	29,405	38	41	29	664		W by N	1,2	Fine.
		3	0	29,457		44	31	634		NW	1	Cloudy.
♃	6	9	0	29,739	34	38	28	712		NW	1,2	Cloudy.
		3	0	29,749		40	21	521		W NW	1	Fine.
♀	7	9	0	29,458	31	37	24	641		SE	1	Cloudy.
		3	0	29,069		32	31	963		E	1,2	Snow.
♄	8	9	0	29,060	32	39	30	735		W	1	Fine.
		3	0	29,082		40	28	671		NW	1	Fine.
☉	9	9	0	29,162	34	43	27	576		N by E	1	Fine.
		3	0	29,354		41	22	521		NE	1	Fine.
☾	10	9	0	29,724	31	37	32	844		N	1	Cloudy.
		3	0	29,626		47	35	659		SE	1,2	Fine.
♀	11	9	0	29,687	36	41	39	930		W	1	Fine.
		3	0	29,763		48	36	660				
☿	12	9	0	30,063	35	41	37	877		N	1	Fine.
		3	0	30,104		49	35	633		NNE	1	Fine.
♃	13	9	0	30,231	36*	44	39	829		W	1	Hazy.
		3	0	30,196		52	45	794				
♀	14	9	0	30,216		52	45	794		W	1	Cloudy and hazy.
		3	0	30,218		53	47	820		W	1	Cloudy and hazy.
♄	15	9	0	30,386		41	38	904		N by E	1	Cloudy and hazy.
		3	0	30,386		45	40	824		N	1	Cloudy.
☉	16	9	0	30,423		38	36	939		N by E	1	Cloudy and foggy.
		3	0	30,318		42	33	737		E	1	Fine.

Register Thermometer deranged.

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☾ 17	9 0	30,109		44	41	890		WNW	1	Cloudy and hazy.
	3 0	30,063		49	43	814		WNW	1	Cloudy and hazy.
♀ 18	9 0	29,788		45	40	823		W	1	Cloudy.
	3 0	29,627		50	43	790		NW	1	Rain.
☿ 19	9 0	29,684		33	32	964		N	1	Cloudy.
	3 0	29,820		38	30	758		N	1	Cloudy.
♃ 20	9 0	29,737		35	33	933		SW	1,2	Snow.
	3 0	29,475		43	37	810	0.540	SW	2	Rain.
♀ 21	9 0	29,296		46	43	898		W	2	Cloudy.
	3 0	29,223		53	47	820		WNW	2	Cloudy.
♄ 22	9 0	29,106		50	43	790		SW	2	Cloudy.
	3 0	29,130		50	42	760		SW	2	Fine.
☉ 23	9 0	29,582		45	38	776		N	1	Cloudy.
	3 0	29,727		47	35	659		N	1	Cloudy.
☾ 24	9 0	30,154		45	36	729		E	1	Cloudy and Hazy.
	3 0	30,185		48	40	745		E	1	Fine.
♀ 25	9 0	30,280		39	37	941		NE	1	Hazy and cloudy.
	3 0	30,191		43	43	1000		NE	1	Fine.
☿ 26	9 0	30,085		42	37	842		NNE	1	Cloudy and hazy.
	3 0	30,038		44	42	927		E	1	Cloudy.
♃ 27	9 0	30,000		42	40	921		Wby N	1	Cloudy and hazy
	3 0	29,974		45	41	859		N	1	Cloudy.
♀ 28	9 0	29,999		42	38	868		NE	1	Hazy.
	3 0	29,989		47	43	868		SE	1	Cloudy.
♄ 29	9 0	30,043		43	40	886		SE	1	Cloudy.
	3 0	30,008		48	42	808		S by E	1	Cloudy.
☉ 30	9 0	29,959		49	40	722		W	1	Cloudy.
	3 0	29,904		57	43	622		SW	1	Fine.
☾ 31	9 0	30,074		50	38	660		NW	1	Fine.
	3 0	30,118		55	42	639		W	1	Cloudy.

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1823 April.	Time. H.	M.	Barometer corrected. Inches.	Register Therm.	Therm. without. °	Daniell's Hygrom. °	Degree of Moisture.	Rain. Inches.	Winds. Points.	Str.	Weather.
♂ 1	9	0	30,031		54	47	791		W by N	1	Fine.
	3	0	29,979		64	43	497		W	1	Fine.
☿ 2	9	0	29,706		57	45	669		SW	1,2	Cloudy.
	3	0	29,629		52	41	682		W	1,2	Cloudy.
♃ 3	9	0	29,779		47	37	703		W	1,2	Cloudy.
	3	0	29,717		52	37	598		W	1	Cloudy.
♀ 4	9	0	29,362		48	45	904		NW	1	Cloudy.
	3	0	29,238		50	45	850		S	1	Rain.
♄ 5	9	0	29,048		51	43	763		S	1	Rain.
	3	0	29,018		51	45	821		NW	1	Rain.
☉ 6	9	0	29,187		43	41	924	0.250	NW	1	Cloudy.
	3	0	29,310		45	37	948		NW	1	Cloudy.
☽ 7	9	0	29,681		42	37	842		N	1	Rain.
	3	0	29,706		45	38	776		NW	1	Cloudy.
♂ 8	9	0	29,844		44	38	971		E	1	Cloudy.
	3	0	29,801		48	37	681		E	1	Fine.
☿ 9	9	0	29,777		41	35	822		N	1	Cloudy.
	3	0	29,814		47	36	681		N by E	1	Fine.
♃ 10	9	0	30,087		46	35	682		E	1	Fine.
	3	0	30,119		48	33	596		SE	1	Fine.
♀ 11	9	0	30,200		40	35	857		E	1	Fine.
	3	0	30,158		50	31	520		E	1	Fine.
♄ 12	9	0	30,170		40	33	800		NE	1	Fine.
	3	0	30,121		45	29	571		N	1	Cloudy.
☉ 13	9	0	30,071		41	35	822		E	1	Cloudy.
	3	0	29,838		45	31	612		E	1	Cloudy.
☽ 14	9	0	30,098		42	32	711		E	1	Cloudy and hazy.
	3	0	30,108		46	33	636		E	1	Cloudy.
♀ 15	9	0	30,332		43	32	684		E	1	Cloudy.
	3	0	30,302		47	36	681		S	1	Fine.

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1823 April.	Time. H.	M.	Barom. corrected. Inches.	Six's Therm. °	Therm. without. °	Daniell's Hygrom. °	Degree of Moisture.	Rain. Inches.	Winds. Points.	Str.	Weather.
☿ 16	9	0	30,206	39	45	37	753		W	1	Cloudy.
	2	0	30,054	52	52	53	738		W	1	Cloudy.
♃ 17	9	0	29,971	48	53	45	766		W	1	Fine.
	2	0	29,949	56	56	44	667		W	1	Fine.
♀ 18	9	0	29,488	44	47	34	637		SE	1	Fine.
	2	0	29,464	52	52	33	523		SW	1	Fine.
♄ 19	9	0	29,477	37	47	29	533		NW	1	Cloudy.
	2	0	29,557	49	45	26	518		NW	1	Cloudy.
☉ 20	9	0	29,896	34	42	38	868		NW	1	Fine.
	2	0	29,937	51	51	27	440		NW	1	Fine.
☾ 21	9	0	29,955	39	45	32	635		NW	1	Fine.
	2	0	29,938	53	51	37	618		NW	1	Cloudy.
♂ 22	9	0	29,771	38	47	35	659		E	1	Fine.
	2	0	29,689	54	54	34	504		SSE	1	Fine.
☿ 23	9	0	29,354	41	45	38	776		SE	1	Cloudy.
	2	0	29,364	55	53	42	685		SSE	1	Cloudy.
♃ 24	9	0	29,528	45	45	36	729		N by E	1	Cloudy.
	2	0	29,733	51	51	37	618		N by E	1	Fine.
♀ 25	9	0	29,870	41	49	28	485		SW	1	Fine.
	3	0	29,805	56	54	41	635		W	1	Cloudy.
♄ 26	9	0	29,672	48	51	45	821		SE	1	Cloudy.
	2	0	29,666	51	48	47	968		NE	1	Rain.
☉ 27	9	0	29,894		39	37	941		SW	1	Fine.
	3	0	29,889		51	37	618		S	1	Fine.
☾ 28	9	0	30,075		40	36	886		SE	1	Fine.
	3	0	30,081	63	62	33	377		N	1	Cloudy.
♂ 29	9	0	30,235	40	46	35	682		N	1	Fine.
	3	0	30,283	56	55	34	487		E	1	Cloudy.
☿ 30	9	0	30,435	40	45	36	729		E	1	Cloudy and hazy.
	3	0	30,387	56	56	37	520	0,793	NW	1	Fine.

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1823 May.	Time. H.	M.	Barometer corrected. Inches.	Six's Therm.	Therm. without. °	Daniell's Hygrom. °	Degree of Moisture.	Rain. Inches.	Winds. Points.	Str.	Weather.
♃ 1	9	0	30,374	42	46	42			W	1	Hazy thick weather.
	3	0	30,305						N by E	1	Fine.
♀ 2	9	0	30,363	53	53	47	820		E	1	Hazy.
	3	0	30,293	66	65	42	463		E by S	1	Fine.
♄ 3	9	0	30,309	48	60	47	650		E	1	Fine.
	3	0	30,269	65	63	45	553		E	1	Fine.
☉ 4	9	0	30,400		47	43	868		E	1	Cloudy.
	3	0	30,348	51	52	38	617		E	1	Fine.
☾ 5	9	0	30,186	41	52	38	617		E	1	Fine.
	3	0	30,066	64	64	42	478		SW	1	Fine.
♂ 6	9	0	29,912	43	54	48	817		NE	1	Fine.
	3	0	29,854	69	67	42	435		E	1	Fine.
☿ 7	9	0	29,772	54	65	47	554		E	1	Fine.
	3	0	29,794	75	73	49	457		W	1	Cloudy.
♃ 8	9	0	29,755	54	59	52	788		S	2	Cloudy.
	3	0	29,685	65	62	47	613		SW	2	Cloudy.
♀ 9	8	0	29,667	46	52	43	738		E	1	Cloudy.
	3	0	29,794	60	59	45	626		W by N	1	Cloudy.
♄ 10	9	0	29,789	53	64	48	591		W	1	Cloudy.
	3	0	29,694	63	62	51	697		SW	1	Cloudy.
☉ 11	9	0	29,666	53	57	48	740		SW	2	Cloudy.
	3	0	29,581	61	59	46	648		S	2	Cloudy.
☾ 12	8	0	29,561	53	59	47	752		W	2	Cloudy.
	3	0	29,610	62	61	50	693		W	2	Cloudy.
♂ 13	8	0	29,594	60	57	42	599		W	1	Cloudy.
	3	0	29,619	64	63	40	455		NW	1	Fine.
☿ 14	8	0	29,704	43	53	42	685		NNW	1	Cloudy.
	3	0	29,794	59	58	30	380				
♃ 15	9	0	29,984	40	56	38	537		NW	1	Fine.
	3	0	30,094	63	62	38	445		W	1	Fine.
♀ 16	9	0	30,073	51	54	45	739		WSW	1,2	Cloudy.
	3	0	30,113	60	60	42	543		WNW	1	Cloudy.

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1823 May	Time. H.	Time. M.	Barometer corrected. Inches.	Six's Therm.	Therm. without. °	Daniell's Hygrom. °	Degree of Moisture.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
♄ 17	9	0	29,843	53	59	52	788		W	2	Cloudy.
	3	0	29,948	65	63	46	572		WNW	1	Fine.
☉ 18	9	0	30,161	44	60	40	500		SW	1	Fine.
	3	0	30,074	67	65	38	411		S	1	Fine.
☾ 19	8	30	29,839	42	53	50	901		E	1	Cloudy and very hazy.
	3	0	29,718	62	62				E by S	1	Cloudy.
♂ 20	9	0	29,634	53	66	48	555		SW	1	Cloudy.
	3	0	29,625	67	66	47	537		S	1	Fine.
☿ 21	8	30	29,559	54	59	43	582		W	2	Fine.
	3	0	29,641	64	63	48	611		S	2	Cloudy.
♃ 22	9	0	29,658	52	57	48	740		S	2	Rain.
	3	0	29,664	63	59	44	604		W	1	Cloudy.
♀ 23	9	0	29,726	52	56	49	789	0.330	WSW	1	Cloudy. Rain in the night.
	3	0	29,800	62	59	44	604			1	
♄ 24	9	0	29,865	48	53	47	820		SW	1	Rain.
	3	0	29,865	65	63	45	553	0.212	SW	1	Cloudy.
☉ 25	9	0	29,639	54	62	48	633		E	1	Cloudy.
	3	0	29,646	65	65	50	609		W	1	Fine.
☾ 26	9	0	29,659	51	64	44	516		SW	1	Cloudy.
	3	0	29,696	65	64	43	497		SSW	1	Fine.
♂ 27	9	0	29,918	50	60	43	564		N	1	Fine.
	3	0	29,924	68	68	47	504		NE	1	Fine.
☿ 28	9	0	30,002	50	61	48	652		NE	1	Fine.
	3	0	29,991	70	68	40	388		NE	1	Fine.
♃ 29	9	0	30,115	48	56	45	691		N	1	Cloudy.
	3	0	30,097	65	65	50	609		NE	1	Fine.
♀ 30	9	0	30,157	52	62	51	697		E	1	Fine.
	3	0	30,133	70	70	50	520		E	1	Fine.
♄ 31	9	0	30,191	54	68	49	537		E	1	Fine.
	3	0	30,179	70	70	47	473		E	1	Fine.

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1823 June.		Time. H.	Time. M.	Barometer corrected. Inches.	Six's Therm. °	Therm. without. °	Daniell's Hygrom. °	Degree of Moisture.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
☉	1	9	0	30,160	52	68	51	573		NNE	1	Fine.
		3	0	30,092		73	49	475		NW	1	Fine.
☾	2	9	0	30,005	59	69	46	473		W	1	Fine.
		3	0	29,712	74	70	55	618		SW	1	Cloudy.
♂	3	9	0	29,601	52	60	46	629		W	1	Fine.
		3	0	29,571	63	63	41	475		NE	1	Cloudy.
☿	4	9	0	29,601	52	59	44	604		SW	1	Fine.
		3	0	29,570	63	63	41	475		SW	1	Fine.
♃	5	9	0	29,478	47	57	42	598		SW	1	Cloudy.
		3	0	29,529	64	56	45	691	0.288	SW	1	Rain.
♀	6	8	0	29,791	43	55	46	739		W	1	Cloudy and hazy.
		3	0	29,900	66	64	38	415		W	1	Cloudy.
♄	7	8	0	30,075	48	59	43	582		W	1	Cloudy.
		3	0	30,098	66	66	48	555		W	1	Cloudy.
☉	8	8	0	29,944	54	62	45	572		W	1	Fine.
		3	0	29,873	68	67	42	435		S	1	Cloudy.
☾	9	9	0	29,870		54				W	1	Cloudy.
		2	0	29,901	67	62	42	512		N	1	Cloudy.
♂	10	9	0	29,943	48	61	42	527		NNE	1	Fine.
		3	0	29,952	65	62	47	613		N	1	Cloudy.
☿	11	9	0	30,055	48	55	47	765		N	1	Fine.
		3	0	30,017	64	63	45	553		N	1	Cloudy.
♃	12	9	0	30,029	49	59	50	737		NE	1	Fine.
		3	0	29,960	68	67	44	469		N	1	Fine.
♀	13	9	0	29,877	51	65	43	481		N	1	Fine.
		3	0	29,811	72	71	41	367		N	1	Fine.
♄	14	9	0	29,825	61	63	50	650		N	1	Fine.
		2	0	29,825	71	63	56	800				
☉	15	8	0	29,957	60	63	45	553		N	1	Cloudy.
		2	0	30,028	68	68	48	521		W	1	Fine.

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1823 June.	Time. H.	Time. M.	Barometer corrected. Inches.	Six's Therm. °	Therm. without. °	Daniell's Hygrom. °	Degree of Moisture.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
☾ 16	9	0	30,268	47	59	42	560		E	1	Fine.
	3	0	30,271	65	65	37	390		N	1	Fine,
♂ 17	9	0	30,296	48	61	42	527		N by E	1	Fine.
	3	0	30,240	70	68	43	438		N	1	Fine.
☿ 18	8	0	30,210	50	52	50	738		N	1	Cloudy.
	2	0	30,137	67	66	67	484		N	1	Cloudy.
♃ 19	9	0	30,147	49	56	49	667		NE	1	Fine.
	3	0	30,093	63	61	63	548		N	1	Fine.
♀ 20	9	0	30,011	50	58	50	650		N	1	Fine, rather hazy.
	3	0	29,968	69	68	69	505		N	1	Cloudy.
♄ 21	9	0	30,069	51	55	43	664		NE	1	Cloudy.
	3	0	30,084	59	56	43	642		NE	1	Cloudy.
☉ 22	9	0	30.133	49	51	40	676		N	1	Cloudy.
	3	0	30,124	55	53	40	602		N by E	1	Cloudy.
☾ 23	9	0	30,051	46	50	38	660		N	1	Fine.
	3	0	29,998	59	56	38	537		N	1	Cloudy.
♂ 24	9	0	29,921	45	56	41	594		W	1	Cloudy.
	3	0	29,861	66	65	40	426		W	1	Cloudy.
☿ 25	9	0	29,725	47	63	40	455		W	1	Fine.
	3	0	29,719	66	63	43	514		W	1	Cloudy.
♃ 26	9	0	29,628	52	64	45	535		SW	1	Fine.
	3	0	29,563	67	66	42	448		S	1	Cloudy.
♀ 27	9	0	29,301	55	60	50	714		E	1	Cloudy; rain in the night.
	3	0	29,238	68	66	49	572	0.255	SW	1	Cloudy; a storm of thunder
♄ 28	9	0	29,316	52	62	50	673	0.250	SW	1	Cloudy. [and hail at 4h 20m P.M.
	3	0	29,315	67	64	52	673		SW	1	Cloudy.
☉ 29	9	0	29,681	52	66	50	590		W	1	Fine. [hail.
	3	0	29,728	67	62	55	801		NW	1	A thunder storm with large
☾ 30	9	0	29,949	52	64	48	591		W by N	1	Fine.
	3	0	29,915	69	68	45	471	0.183	SSE	1	Cloudy.

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1823 July.	Time. H. M.	Barometer corrected. Inches.	Six's Therm. °	Therm. without. °	Daniell's Hygrom. °	Degree of Moisture.	Rain. Inches.	Winds. Points.	Str.	Weather.
♂ 1	9 0	29,913	56	62	48	633		N by E	1	Cloudy and hazy.
	3 0	29,958	57	61	50	677		W	1	Rain.
☿ 2	9 0	30,002	52	65	48	572	0.087	W	1	Fine.
	3 0	29,993	70	68	47	504		N	1	Fine.
♃ 3	9 0	30,049	53	57	51	815		N by E	1	Cloudy.
	3 0	30,057	68	67	47	521		E	1	Fine.
♀ 4	9 0	30,008	56	57	50	787		SSE	1	Rain.
	3 0	29,932	63	61	50	693	0.023	S	1	Cloudy.
♄ 5	9 0	30,016	54	66	50	590		SW	1	Cloudy.
	3 0	29,904	73	71	51	598		W	1	Fine.
☉ 6	9 0	29,717	62	67	54	658		E	1,2	Cloudy.
	3 0	29,679	69½	69	51	556		W	1,2	Fine.
☾ 7	8 30	29,797	54	62	47	613		WSW	1	Fine.
	3 0	29,665	69	60	51	713		SW	1	Rain.
♂ 8	9 0	29,669	52	59	47	670	0.408	W	1	Cloudy.
	3 0	29,652	64	63	47	591		W	1	Fine.
☿ 9	9 0	29,998	49	62	49	653		W	1	Fine.
	3 0	29,949	70	69	43	424		W	1	Fine.
♃ 10	8 0	30,053	53	65	51	630		SW	1	Fine.
	3 0	29,991	71½	70	49	504		S	1	Cloudy.
♀ 11	9 0	29,708	61	70	52	556		S	1	Fine.
	3 0	29,660	73	71	56	618		W	1	Fine.
♄ 12	9 0	29,699	59	64	51	651		S	2	Cloudy.
	3 0	29,726	70	69	47	489		SSW	1,2	Fine.
☉ 13	8 30	29,745	60	67	52	612		S	2	Cloudy.
	2 30	29,702	69	65	54	700		S	2	Cloudy.
☾ 14	9 0	29,695	56	67	52	612		W	1	Fine.
	3 0	29,717	69	68	55	659	0.143	S	1	Rain.
♂ 15	9 0	29,759	54	66	48	555		S	1,2	Cloudy.
	3 30	29,570	68	61	55	825		S by E	2	Rain.
☿ 16	9 0	29,614	54	63	45	553		W	1	Cloudy.
	2 0	29,585		66	47	537		W	1	Fine.

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1823 July.	Time.	Barom. corrected.	Six's Therm.	Therm. without.	Daniell's Hygrom.	Degree of Moisture.	Rain.	Winds.		Weather.
	H. M.	Inches.	°	°	°		Inches.	Points.	Str.	
♃ 17	8 0	29,831	50	60	43	564		W	1	Cloudy.
	5 0	29,895	66	63	47	592				
♀ 18	9 0	29,778	55	60	52	764		S by E	1	Rain.
	3 0	29,765	66	65	50	609		W	1	Cloudy.
♄ 19	9 0	29,939	53	57	53	874		W	1	Rain.
	3 0	29,909	65	61	53	769		SSW	1	Rain.
☉ 20	8 0	29,976	61	70	59	705	0.102	W	1	Fine.
	3 0	29,963	74	72	60	681	0.162	SSW	1	Cloudy.
☾ 21	9 0	29,647	69	68	57	704		S	1	Cloudy.
	3 0	29,472	69	68	60	775		SSW	1	Cloudy.
♂ 22	8 30	29,813	52	63	47	592		W	1	Fine.
	3 0	29,837	70	67	48	538		W	1	Fine; a thunder storm at 4½ P. M.
☿ 23	9 0	29,619	57	60	53	793		S	2	Rain.
	3 0	29,489	69	68	51	573		W	1	Fine.
♃ 24	9 0	29,671	55	63	50	650		W	1	Cloudy.
	2 30	29,766	68	67	48	538		N	1	Fine.
♀ 25	8 0	29,816	51	51	49	937		W	1	Cloudy.
	3 0	29,737	66	63	49	631		S	1,2	Cloudy.
♄ 26	9 0	29,615	53	58	51	787		W	1	Rain.
	3 0	29,662	65	63	50	650		N	1	Cloudy.
☉ 27	9 0	29,812	53	57	43	622		N	1	Cloudy.
	3 0	29,874	66	65	52	651	0.128	W	1	Cloudy.
☾ 28	9 0	29,847	56	61	51	717		SW	1,2	Cloudy.
	3 0	29,822	70	69	53	596		W	1	Cloudy.
♂ 29	9 0	29,809	59	62	50	673		W	1	Rain.
	3 0	29,759		66	51	611		W	1	Cloudy.
☿ 30	9 0	29,795	49	67	48	538	0.098	SW	1	Fine.
	3 0	29,789	70	68	50	554		S	1	Cloudy.
♃ 31	9 0	29,903	58	65	53	676*		W by N	1	Fine.
	3 0	29,954	70	68	46	487		NW	1	Fine.

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1823 August.		Time.		Barom. corrected.	Six's Therm.	Therm. without.	Daniell's Hygrom.	Degree of Moisture.	Rain.	Winds.		Weather.
		H.	M.	Inches.	°	°	°		Inches.	Points.	Str.	
♀	1	9	0	30,107	55	62	46	579		S	1	Cloudy.
		3	0	30,070	69	68	48	521		SW	1	Cloudy.
♄	2	9	0	30,000	58	63	53	722		SW	2	Rain.
		3	0	30,016	69	69	51	556		S	1	Fine.
☉	3	8	0	29,838	62	66	57	749		SW	1,2	Cloudy.
		3	0	29,755	67	65	58	801		S	2	Rain.
☾	4	9	0	29,654	56	67	53	635		W	2	Fine.
		3	0	29,619	72	72	53	540		W	1	Fine.
♂	5	9	0	29,808	58	64	45	535		W	1	Cloudy.
		3	0	29,799	69	64	49	610	0.427	W	1	Showery.
☿	6	9	0	29,785	51	64	43	497		W	1	Fine.
		3	0	29,769	67	64	47	572		W	1	Fine.
♃	7	9	0	29,865	50	63	46	572		NW	1	Fine.
		3	0	29,827	66	64	49	610		W	1	Cloudy.
♀	8	9	0	29,796	55	64	47	572		W	1	Fine.
		3	0									
♄	9	9	0	29,926	50	61	45	589		NW	1	Cloudy.
		3	0	29,968	67	64	48	591		NW	1	Fine.
☉	10	9	0	30,126	49	60	47	650		SW	2	Rain.
		2	0	30,073		62	56	828		W	2	Rain.
☾	11	9	0	30,040	61	68	59	682		W	1	Cloudy.
		3	0	29,983	73	72	60	681		W	1	Cloudy.
♂	12	9	0	29,905	62	69	60	752		W	1	Fine.
		3	0	29,870	74	73	57	598		W	1	Fine.
☿	13	8	0	29,663	63	68	58	729		W	1	Fine.
		2	0	29,694	78	75	58	581	0.061	W	1	Fine.
♃	14	9	0	29,635	60	63	47	592		W	1	Cloudy.
		3	0	29,758	66	65				NW	1	Fine.
♀	15	9	0	29,819	52	64	46	554		SSW	1	Fine.
		3	0	29,697	62	62	54	774		S	1,2	Cloudy.
♄	16	9	0	29,532	51	61	49	673		S	1,2	Showery.
		5	0	29,464	66	59	47	670		W	2	Cloudy.

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	H.	M.	Inches.		°	°		Inches.	Points.	Str.	
☉ 17	8	0	29,852	50	62	47	613		W	1	Fine.
	3	0	29,881	66	65	47	542		SSW	1	Cloudy.
☾ 18	9	0	29,888	56	63	52	698		SE	1	Fine.
	3	0	29,826	65	62	53	748		SSE	1	Cloudy.
♂ 19	9	0	29,811	62	66	56	726		S	2	Cloudy.
	3	0	29,811	69	65	61	878		SW	1,2	Rain.
☿ 20	9	0	29,863	56	64	50	629	0.114	W	1	Fine.
	3	0	29,809	69	66	49	572		W	1	Fine.
♃ 21	9	0	29,859	52	61	49	672		W	1	Fine.
	3	0	29,871	69	66	47	537		W	1	Fine.
♀ 22	9	0	29,780	55	60	55	850		E	1	Cloudy.
	3	0	29,701	67	64	57	799		S	2	Rain.
♄ 23	9	0	29,711	61	66	55	702	0.186	W	1	Cloudy.
	3	0	29,737	70	67	60	801		WSW	1	Cloudy.
☉ 24	9	0	29,860	61	66	58	776		SSW	1	Cloudy.
	3	0	29,845	69	69	61	775		SSW	1	Cloudy.
☾ 25	9	0	29,801	61	65	60	852		W	1	Cloudy and hazy
	3	0	29,803	75	75	63	679	0.250	SW	1	Fine.
♂ 26	9	0	29,999	61	63	60	912		E	1	Showery.
	3	0	29,958	67	66	61	851	0.011	E	1	Cloudy.
☿ 27	9	0	30,103	61	65	57	773		N	1	Cloudy.
	3	0	30,098	73	72	60	681		S	1	Cloudy.
♃ 28	9	0	30,195	61	64	54	723		W	1	Fine.
	3	0	30,150	72	72	56	599		W	1	Cloudy.
♀ 29	8	0	30,053	59	62	52	721		WSW	1	Cloudy.
	3	0	29,952	73	72	57	618		W	1	Cloudy.
♄ 30	9	0	29,988	61	63	52	712		NNE	1	Cloudy.
	3	0	29,957	67	66	57	749		N by E	1	Rain.
☉ 31	9	0	30,189	54	60	52	764		W	1	Fine.
	3	0	30,207	70	69	52	574		W	1	Fine.

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1823 September.	Time. H.	Time. M.	Barometer corrected. Inches.	Six's Therm. °	Therm. without. °	Daniell's Hygrom. °	Degree of Moisture.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
☾ 1	9	0	30,231	53	63	51	672		W	1	Fine.
	3	0	30,159	71	69	52	574	0.048	W	1	Fine.
♂ 2	9	0	30,067	56	64	53	698		SW	1	Fine.
	3	0	29,995	71	71	53	558		W	2	Fine.
☿ 3	9	0	30,055	52	60	50	714		W	1	Cloudy.
	3	0	30,063	66	65	50	609		NW	1	Cloudy.
♃ 4	9	0	30,169	59	64	56	774		W	1	Fine.
	3	0	30,159	73	71	53	558		WNW	1	Fine.
♀ 5	9	0	30,129	54	62	53	747		W	1,2	Fine.
	3	0	30,082	72	71	58	661		W	1	Fine.
♄ 6	9	0	30,168	55	63	48	611		NW	1	Fine.
	3	0	30,104	66	65	48	572		N	1	Fine.
☉ 7	8	0	30,164	48½	54	48	817		N	1	Fine.
	3	0	30,186	64	63	43	514		E	1	Fine.
☾ 8	9	0	30,274	45	55	43	664		NNE	1	Fine.
	3	0	30,244	62	59	42	560		E	1	Fine.
♂ 9	9	0	30,230	44	53	39	613		NNE	1	Fine.
	3	0	30,177	61	61	42	527		N by E	1	Fine.
☿ 10	9	0	30,213	45	50	43	790		NNE	1	Hazy.
	3	0	30,173	64	64	45	535		E by S	1	Fine.
♃ 11	9	0	30,275	50	60	48	671		E	1	Cloudy.
	2	0	30,249	68	67	50	572		E	1	Fine.
♀ 12	9	0	30,112	51	59	52	788		E	1	Fine.
	3	0	29,957	68	68	51	573		SSE	1	Fine.
♄ 13	9	0	29,877	58	66	55	702		W	1	Cloudy.
	3	0	29,823	73	71	57	638		W	1	Cloudy.
☉ 14	8	30	29,749	62	64	57	799		W	1,2	Cloudy.
	3	0	29,660	71	69	58	706		SW	1	Fine.
☾ 15	9	0	29,406	64	62	53	747	0.065	W	2	Showery.*
	3	0	29,578	68	64	48	591		SSW	2	Fine.
♂ 16	9	0	29,832	52	59	47	670		W	1	Fine.
	3	0	29,873	68	65	44	499		SW	1	Fine.

* Storm of hail, wind, and lightning, at 4 A. M. Bar. 29.340. Ther. 66.

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☿	17	9 0	29,888	55	60	52	764	0.015	S	1	Cloudy.
		3 0	29,915	65	63	55	774		NW	1	Cloudy.
♃	18	9 0	30,343	47	54	47	791		N	1	Fine.
		3 0	30,352	62	61	46	610		N by W	1	Fine.
♀	19	9 0	30,292	47	54	45	739		W	1	Fine.
		2 30	30,179	63	63	46	572		W	1	Cloudy.
♄	20	9 0	30,168	53	56	47	740		N	1	Fine.
		3 0	30,103	61	60	48	672		W	1	Fine.
☉	21	9 0	29,863	48	59	46	648		W	1,2	Cloudy.
		3 0	29,667	59	56	50	813		SW	2	Rain.
☾	22	9 0	29,289	57	56	45	691		SW	2	Cloudy.
		3 0	29,353	57	53	48	847	0.128	N	1	Rain.
♂	23	9 0	29,953	44	50	42	760		NW	1	Fine.
		3 0	29,952	56	56	44	667		W by N	1	Cloudy.
☿	24	9 0	29,849	52	59	51	762	0.272	W	1	Cloudy.
		3 0	29,868	67	60	51	659		W	1	Cloudy.
♃	25	9 0	30,006	55	60	52	764		W	1	Cloudy.
		3 0	29,973	62	61	51	717		W	1	Cloudy.
♀	26	9 0	29,847	56	60	51	739		S	1,2	Fine.
		3 0	29,770	66	65	48	572		SW	1	Cloudy.
♄	27	9 0	29,811	49	52	43	738		NW	1	Fine.
		3 0	29,797	57	55	42	639		NW	1	Fine.
☉	28	9 0	29,900	41	46	40	795		W	1	Hazy.
		2 30	29,903	54	54	41	635		W	1	Fine.
☾	29	9 0	30,009	40	47	42	835		NNE	1	Fine.
		3 0	29,891	56	56	41	594		E	1	Cloudy.
♂	30	9 0	29,399	44	51	46	850		S	2	Cloudy.
		3 0	29,259	52	51	45	821	0.270	NNW	1,2	Fine

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1823 October.		Time. H. M.	Barom. corrected. Inches.	Six's Therm. °	Therm. without. °	Daniell's Hygrom. °	Degree of Moisture.	Rain. Inches.	Winds. Points.	Str.	Weather.
☿	1	9 0	28,698	45	55	49	815		S	2	Rain.
		3 0	28,923	59	44	42	927	0.753	N	1	Rain.
♃	2	9 0	29,409	38	42	40	921	0.178	W	1	Fine.
		3 0	29,484	50	48	40	745		N	1	Fine.
♀	3	9 0	29,849	37	44	40	854		W	1	Fine.
♄	4	9 0	29,966	39	52	45	794		NW	1	Fine.
		3 0	30,048	56	55	43	664		NW	1	Fine.
☉	5	9 0	30,084	46	54	44	713		SSE	1	Cloudy.
		3 0	29,999	60	59	51	762		S	2	Cloudy.
☾	6	9 0	29,847	57	59	54	847		S	1,2	Fine.
		3 0	29,819	62	58½	53	844		S	2	Rain.
♂	7	9 0	29,936	52	57	50	806		W	1	Cloudy.
		3 0	29,944	59	57	48	740		W	1	Fine.
☿	8	9 0	30,073	45	48	43	840		W	1,2	Hazy.
		2 30	29,960	59	57	47	717		W	1,2	Fine.
♃	9	9 0	29,526	50	51	47	879		W	1	Cloudy.
		2 30	29,541	56	54	42	661		NW	2	Cloudy.
♀	10	9 0	29,424	42	47	32	835		S	1	Fine.
		2 30	29,320	47					WNW	1	Showery.
♄	11	9 0	28,997	47	49	42	783	0.133	SW	2,3	Fine.
		3 0	29,034	54	52	41	682		SSW	1	Fine.
☉	12	8 0	29,169	43	47	43	868		W	1	Cloudy.
		3 0	29,249	53	52	42	710		S	1	Cloudy.
☾	13	9 0	29,227	43	49	44	845		E	1	Rain.
		3 0	29,275	54	54	42	661		SW	2	Cloudy.
♂	14	9 0	29,499	40	43	38	835		SW	1	Hazy.
		3 0	29,495	50	50	43	790		W	1	Cloudy.
☿	15	9 0	29,439	40½	44	39	829		SSW	1	Cloudy..
		3 0	29,405	52	51	45	821		W	1	Fine.
♃	16	9 0	29,575	40	47	43	868		W	1	Fine.
		2 30	29,509	54	52	36	584		W	1	Cloudy.

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♀ 17	9	0	29,595	39	45	42	894		SW	1	Fine.
	2	0	29,553	53	52	39	636		W	1	Fine.
♄ 18	9	0	29,456	44	48	43	840		E	1	Rain and fog.
	3	0	29,451	56	56	49	789		S by E	1	Cloudy.
☉ 19	8	30	29,638	50	52	47	891		E	1	Cloudy and hazy.
	2	30	29,663	58	58	46	669		E	1	Cloudy.
☾ 20	9	0	29,979	52	56	48	764		E	1	Cloudy.
	2	0	30,029	60	59	48	692		E	1	Fine.
♂ 21	9	0	30,235	53	55	49	815		E	1,2	Cloudy.
	3	0	30,201	57	56	51	841		E	1	Cloudy.
☿ 22	9	0	30,171	52	52	46	822	0.195	E	1	Cloudy.
	2	0	30,099	55	54	45	739		E	1	Fine.
♃ 23	9	0	30,008	45	49	45	876		E	1	Fine.
	3	0	29,934	55	53	43	712		E	1	Fine.
♀ 24	9	0	30,008	46	50	42	760		E	1	Fine.
	3	0	30,037	54½	53	43	712		E	1	Fine.
♄ 25	9	0	30,275	41	46	43	898		N	1	Fine, rather hazy.
	3	0	30,218	49	48	47	968		NE	1	Cloudy.
☉ 26	8	0	30,452	45½	47	38	725		NE	1	Cloudy and hazy.
	3	0	30,430	49	49	40	738		W	1	Cloudy dull weather.
☾ 27	9	0	30,250	45	47	42	835		W	1	Cloudy.
	3	0	30,144	50	50	46	880		W	1	Cloudy.
♂ 28	9	0	29,835	48	51	47	879		S by E	1	Cloudy.
	3	0	29,674	55	55	45	714		S	1	Cloudy.
☿ 29	9	0	29,674	43	45	40	824		W	1	Fine.
	3	0	29,661	52	51	39	657	0.088	W	1	Cloudy.
♃ 30	9	0	29,249	44	45	42	894		ENE	1	Rain.
	3	0	29,118	46	46	45	966	1.000	E	1	Cloudy.
♀ 31	9	0	29,038	41	41	39	932		NE	2	Rain.
	3	0	29,193	43	40	38	943	0.165	NE	2	Rain.

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♄ 1	9	0	29,741	40	42	37	842		N	2	Cloudy.
	2	0	29,838	44	41	31	877		N	2	Cloudy.
☉ 2	8	30	30,090	33	34	29	836		N	1	Hazy.
	3	0	30,092	42	41	32	740		N	1	Fine.
☾ 3	9	0	30,017	32	39	36	912		W	1	Cloudy and hazy.
	3	0	29,914	47	47	39	747		SW	1	Cloudy.
♂ 4	9	0	29,659	46	49	44	845		S	2,3	Cloudy.
	3	0	29,560	51	49	40	722		SE	2	Cloudy.
☿ 5	9	0	29,668	45	47	41	802		E	1	Rain.
	2	30	29,632	49	49	44	845	0.219	E	1	Rain.
♃ 6	9	0	29,915	48	52	48	879		ESE	1	Cloudy.
	3	0	29,900	55	54	48	817		E	1	Fine.
♀ 7	9	0	29,941	50	53	50	901		N	1	Rain and thick fog.
	3	0	30,002	55	54	51	900	0.480	N	1	Cloudy.
♄ 8	9	0	30,175	51	52	51	967		NE	1	Cloudy.
	3	0	30,219	49	51	49	937		NNE	1	Cloudy.
☉ 9	9	0	30,043	39	43	39	861		E	1	Fine.
	3	0									
☾ 10	9	0	30,543	39	43	39	861		E	1	Cloudy.
	3	0	30,569	40	41	40	959		E	1	Fine.
♂ 11	9	0	30,625	35	40	35	817		E	1	Cloudy.
	3	0	30,574	36	41	36	849		SE	1	Cloudy.
☿ 12	9	0	30,480	29	33	29	866		S	1	Thick fog.
	3	0	30,413	30	38	30	758		SSE	1	Fine.
♃ 13	9	0	30,431	32	35	31	867		SE	1	Fine.
	3	0	30,393	44	43	35	759		SE	1	Fine.
♀ 14	9	0	30,328	32	36	33	903		W	1	Hazy.
	3	0	30,227	42	41	35	822		W	1	Fine.
♄ 15	9	0	30,274	41	46	40	769		N by E	1	Cloudy and foggy.
	3	0	30,294	49	48	43	840		N	1	Cloudy.

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1823 November.		Time.		Barometer corrected.	Six's Therm.	Therm. without.	Daniell's Hygrom.	Degree of Moisture.	Rain.	Winds.		Weather.
		H.	M.	Inches.	°	°	°		Inches.	Points.	Str.	
☉	16	9	0	30,427	43	43	42	962		N	1	Rather hazy. [hazy
		3	0	30,392	48	47	43	868		N	1	Fine, though somewhat
☾	17	9	0	30,399	37	39	38	970		N	1	Foggy.
		3	0	30,336	42	42	42	1000		N	1	Cloudy and foggy.
♂	18	9	0	30,429	42	44	42	927		N	1	Cloudy and foggy.
		3	0	30,407	47	44	39	829		E	1	Cloudy.
☿	19	9	0	30,204	43	45	38	776		NE	1	Cloudy.
		3	0	30,141	44	45	42	894		NE	1	Cloudy.
♃	20	9	0	30,074	43	48	43	840		W	1	Fine.
		3	0	30,074	51	42	40	921		W	1	Fine.
♀	21	9	0	30,187	42	47	43	868		W	2	Cloudy.
		3	0	30,142	49	48	46	936		W	2	Cloudy.
♄	22	9	0	29,978	46	47	43	868		W	1,2	Cloudy.
		2	0	30,622	49	49	44	845		SSW	2	Cloudy.
☉	23	8	30	29,988	44	46	44	932		SW	1	Cloudy.
		2	30	29,988	47½	47	45	934		W by S	1	Cloudy.
☾	24	8	0	30,041	46	47	44	901		SW	1	Cloudy.
		3	0	30,057	50	50	45	850		W	1	Cloudy.
♂	25	9	0	30,210	46	48	46	936		W	1	Cloudy and hazy.
		3	0	30,210	50	49	43	814		W	1	Cloudy and hazy.
☿	26	9	0	30,246	47	48	41	777		W	1	Cloudy and hazy.
		2	0	30,209	48	48	41	777		W	1	Cloudy and hazy.
♃	27	9	0	30,190	46	47	40	769		SSW	1	Cloudy and dark.
		3	0	30,143	48	47	44	901		S	1	Cloudy.
♀	28	9	0	30,013	46	48	43	840		SW	1,2	Cloudy; rain in the night.
		3	0	29,934	49	49	46	907		SW	2	Rain.
♄	29	9	0	29,678	47	48	44	872	0,305	SSE	1	Rain.
		3	0	29,518	52	52	48	879		S	2	Cloudy.
☉	30	9	0	29,464	50	51	47	879		SW	1	Fine.
		3	0	29,423	50	51	50	966	0.155	SW	2	Cloudy.

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1823 December.		Time. H.	M.	Barometer corrected. Inches.	Six's Therm. °	Therm. without. °	Daniell's Hygrom. °	Degree of Moisture.	Rain. Inches.	Winds. Points.	Str.	Weather.
☾	1	9	0	29,861	52	52	48	879		W	1 2	Cloudy.
		3	0	29,807	53	51	43	763		W	1	Fine.
♂	2	9	0	29,504	44	51	48	908		S	2	Rain.
		2	0	29,492	52	50	42	760	0.210	W	2	Fine.
☿	3	9	0	29,559	38	43	39	861		W	2	Fine.
		3	0	29,618	48	46	30	568		W	2	Fine. At midnight a violent gale
♃	4	9	0	29,339	44	45	40	824		W	1,2	Fine. [of wind. Bar. Corr. 29.080.
		3	0	29,474	47	44	38	805		W	1	Fine.
♀	5	9	0	29,854	36½	38	33	848		W	1	Hazy.
		3	0	29,800	42	42	36	816		W	1	Cloudy.
♄	6	9	0	29,508	40	41	37	877	0.316	NNE	1	Rain.
		3	0	29,789	42	41	37	877		N	1	Cloudy.
☉	7	8	30	30,482	33	34	30	862		W	1	Hazy.
		3	0	30,517	37	37	34	907		W	1	Fine.
☾	8	9	0	30,447	33½	39	36	912		W	1	Hazy.
		3	0	30,447	43	43	40	886		W	1	Cloudy.
♂	9	9	0	30,375	39	39	35	882		NNW	1	Fine.
		3	0	30,352	41	41	36	849		NE	1	Fine.
☿	10	9	0	30,325	31	34	32	931		W	1	Hazy.
		3	0	30,365	38½	38½	33	966		W	1	Fine, rather hazy.
♃	11	9	0	30,119	38	44	39	829		NW	1	Cloudy.
		2	30	30,064	47					W	1	Cloudy.
♀	12	9	0	29,816	41	43	38	835		W	1	Fine.
		3	0	29,755	43	41	30	685		WNW	1	Fine.
♄	13	8	0	29,895	34	35	30½	833		NNW	1,2	Fine.
		3	0	30,413	40	40	32	771		W	1	Fine.
☉	14	8	0	30,231	30	30	29	970		W	1	Fine.
		3	0	30,211	37	37	33	875		W		Cloudy.
☽	15	9	0	30,196	37	39	30	735		NW	1	Hazy.
		2	0	30,237	42	42	34	763		W	1	Fine.
♂	16	9	0	30,196	36	42	38	868		W	2	Cloudy.
		4	0	30,033	45	44	38	805		SW	1,2	Cloudy.

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1823 December.	Time. H. M.	Barom. corrected. Inches.	Six's Therm. °	Therm. without. °	Daniell's Hygrom. °	Degree of Moisture.	Rain. Inches.	Winds. Points.	Str.	Weather.
☿ 17	9 0	29,632	42	45	43	929		S	1,2	Rain.
	2 0	29,347	47	47	43	868		S	2	Rain. At 8¼ P.M. a violent gale. Bar. Corr. 28.828.
♃ 18	8 0	29,385	35	35	29	808	0.098	W	1,2	Fine.
	3 0	29,433	41					W	1,2	Fine.
♀ 19	9 0	29,610	32	34	31	897		N	1	Cloudy.
	3 0	29,720	35	33	29	866		W by N	1	Fine.
♄ 20	9 0	29,321	29	38	35	909		E	1	Rain.
	2 0	29,140						S	1,2	Fine.
☉ 21	9 0	29,223	37	38	33	848		W	1	Fine.
	3 0	29,273	40	40	35	857		W	1	Fine.
☽ 22	9 0	29,584	37	39	37	941	0.047	NW	1	Cloudy and hazy.
	3 0	29,703	41½	41	35	822		N	1	Cloudy.
♂ 23	9 0	29,783	35	39	36	912		W	1	Rain.
	3 0	29,709	42	43	41	924		W	1	Cloudy.
☿ 24	9 0	29,950	42	42	39	895		SW	1	Rain.
	3 0	29,948	47	47	45	934		W	1	Cloudy.
♃ 25	8 0	29,979	46	46	43	898		W	1	Cloudy.
	3 0	29,897	49	49	46	907	0.330	SW	1	Cloudy.
♀ 26	9 0	29,815	42	42	40	921		W	1	Fine.
	2 0	29,733	46	46	43	898		SW	1	Fine.
☉ 27	9 0	29,222	44	44	43	963		SW	1	Cloudy.
	3 0	29,209	45	44	42	927		W	1	Fine.
☽ 28	8 30	29,503	37	39	36	912	0.103	SW	1	Fine.
	2 0	29,263	47	47	43	868		W	1	Rain.
♂ 29	9 0	29,208	42	44	39	829		SW	1	Fine.
	3 0	29,175	50	45	43	929		SW	2	Rain.
☿ 30	9 0	29,138	40	44	40	854		W	1,2	Fine.
	3 0	29,159	45	44	38	805		W	1,2	Cloudy.
♃ 31	9 0	29,438	39	40	38	943		W	1	Hazy.
	3 0	29,622	46	45	40	824		WbyN	1	Cloudy.

1823.	Height of Barometer,* corrected.			Height of Thermometer without.			Degrees of Moisture by Daniell's Hygrometer.			Rain.†
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.
January	30,181	29,047	29,673	47	22	34,2	1000	575	778	0,608
February	30,204	28,586	29,438	50	31	41,0	1000	614	813	1,982
March	30,423	29,060	29,803	55	32	43,6	1000	517	767	0,937
April	30,435	29,018	29,831	64	39	48,2	971	377	694	1,043
May	30,400	29,559	29,903	73	46	60,4	901	380	611	0,542
June	30,296	29,238	29,883	73	50	61,8	801	367	573	0,976
July	30,057	29,472	29,802	72	51	64,4	937	424	639	1,151
August	30,195	29,464	29,876	75	59	65,4	912	497	665	1,049
September	30,352	29,289	29,967	71	50	60,0	850	499	682	0,798
October	30,452	29,698	29,688	59	40	50,7	966	584	799	2,512
November	30,625	29,423	29,113	54	33	45,6	1000	722	864	1,159
December	30,517	29,138	30,761	52	30	41,8	970	568	862	1,104
Whole year			29,811			51,4			729	13,857

* The quicksilver in the bason of the barometer is 100 feet above the level of low water spring tides at Somerset-place.

† The Rain Gage is 114 feet above the same level, and 75 feet above the surrounding ground.